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ORIGINAL COMMUNICATIONS.

Experimental Researches applied to Physiology and Pathology.

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I.—ON THE SOURCE OF THE VITAL PROPERTIES.

I think that every tissue possesses its vital properties, in consequence of its peculiar organization, and that in a completely developed animal, nutrition is the source of the vital properties, inasmuch as it is the cause of the maintenance of organization.

I will try to prove the correctness of my opinion, by the following remarks on some of the vital properties of the spinal cord, the nerves, and the muscles.

[* The paper which we have the pleasure of presenting to our readers from Dr. Brown Séquard, is a resumé of many researches made by the author, a part of which only have hitherto been published in any of the foreign journals.

The conclusions arrived at are the result of eight years exclusive devotion to the experiments upon which they are based.—EDS. EX.]

a.—*Source of the reflex faculty in the spinal cord.*

Notwithstanding the experiments of Redi, Whytt, Prochaska, Unzer, Sénac, Fontana, Caldani, Sir G. Blane, Fray, Legallois and many other experimenters; and notwithstanding the much more important experiments of Marshall Hall, Müller, Grainger, Volkmann, Kürschner, Pickford, de Martino, Buchner, Mayer, Paton and Stilling, the existence of the reflex faculty, after the spinal cord has been separated from the encephalon, is not considered by all physiologists as a proof of the independence of the spinal cord. J. W. Arnold and Flourens still maintain that the medulla oblongata is a centre, giving life to the other parts of the nervous system. The reflex faculty possessed by the spinal cord after it has been separated from the encephalon, is considered by J. W. Arnold as a remainder of something given to the spinal marrow by the encephalon, before their separation.

My experiments prove the incorrectness of that opinion.* I have found that after having exhausted the reflex faculty by putting it in action, energetically and frequently, in an animal on whom the spinal cord is separated from the encephalon, it reappears, and becomes soon as energetic as before, provided that the circulation of blood takes place in the cord. Moreover I have found, that if the reflex faculty is put in action frequently, it is able to produce an immense quantity of action: thus, for instance, it can stimulate sufficiently the muscles of a frog's leg to make them raise, in an hour and in divided portions, about twelve pounds, to the height of about two lines. In a pigeon the reflex faculty is able to stimulate the muscles of a leg so far as to make them raise fifty pounds, by fractions, in an hour, to the height of more than one inch.†

I shall add two other decisive proofs:—1. The reflex faculty is very weak in frogs immediately after the spinal cord has been separated from the medulla oblongata, and it increases afterwards, as R. Whytt and Marshall Hall have discovered. I have stated that it increases so much that the posterior limbs are able

* See:—Recherches et expériences sur la physiologie de la moelle épimère. Thèse inaugurale. Paris, 3 Janvier, 1846.—Comptes rendus des séances de l'Académie des Sciences. Paris, 1847 T. xxiv. p. 849.

† See Gaz. Méd. de Paris. T. 4. 1849. p. 233.

to draw up, by reflex action, more than double the weight the animal could raise up by an action of its will before the division of the cord. 2. After having divided the spinal cord in the dorsal region on a mammal, I kill it by cutting the right carotid artery. A few minutes after the cessation of reflex action I inject blood by the opening made in the carotid. Then life returns and with it the reflex faculty.

All these facts demonstrate positively that the reflex faculty is a vital property belonging to the spinal cord, and that its source is in the nutrition which maintains the organization of that nervous centre.

b.—*Source of the vital property of the motor nerves.*

The independence of the motor nerves is denied by almost all physiologists. They believe that the nervous centres are the sources of the vital property of these nerves. They base their opinion on this fact, that the motor nerves separated from the nervous centres soon lose their property, as it has been seen by Fontana, Haighton, Astley Cooper, Steinrueck, Müller, Sticker, Günther, Schoen, Kilian, Stannius, Helmholtz, Martin-Magron and others.

But, *in the first place*, if the motor nerves of the warm-blooded animals lose their vital property after having been separated from the nervous centres, it is not less positive that they retain it during several days. *Secondly*, if the vital property of the motor nerves is exhausted by very energetic action, it reappears after a short time, although the nerves are separated from the cerebro-rachidian centre, provided that the circulation of blood continues in them. *Thirdly*, if the circulation of blood is stopped in a limb in which the nerves have been divided, it is found that the peripheric portion of the divided nerves lose their vital property before the muscles. After the nerves have been left dead, *i. e.*, deprived of their vital property for a quarter of an hour, half an hour, and even more, blood is allowed to circulate anew in the limb. Then the vital property of the cut nerves returns, and, to produce a muscular contraction, only a slight compression upon them is necessary.* If the

* See Comptes rendus de l'Acad. des Sciences. T. xxxii. Séance du 9 Juin, 1851.—Gaz. Médic. de Paris. 1851. T. vi, p. 359.

motor nerves lose their property when they are separated from the nervous centres, it is because they are then badly nourished. Nerves as well as muscles must be exercised, in order to be well nourished.

c.—*Source of the muscular contractility.*

Although there are some facts which appear strongly to prove that the vital property of the muscular tissue is independent of the nervous system, many physiologists persist in their opposition to Haller's doctrine on this subject. Therefore I have thought necessary to add new proofs to those already known, and I have published many experiments, of which I shall relate here only two of the most decisive.*

1. The sciatic and the crural nerves having been resected, for ten or twelve days, on a rabbit or a guinea-pig, I examine if these nerves have completely lost their vital property, and if the muscles are still contractile. When this has been ascertained, I put a ligature around the aorta. Then muscular irritability disappears after a certain time and cadaveric rigidity appears. Three quarters of an hour or even an hour after the complete disappearance of the muscular irritability, and the appearance of the *rigor mortis*, I cut off the ligature, and I find, after ten or fifteen minutes, that the rigidity disappears and the contractility reappears. I need not say that the nerves do not regain their lost property. This fact clearly proves that the contractility is given to the muscles by blood, *i. e.*, by nutrition, and not by the nervous system.

2. Many experiments have shown to me that muscles paralyzed for five days or a little more, in consequence of the division of their nerves, remain much longer contractile after the death of the animal than the non-palsied muscles. This would hardly be the case if the contractility was given to muscles by the nervous system.

* See:—Bulletin de la Soc. Philomat. 1847, p. 74.—Gaz. Méd. de Paris, 1851, t. vi. p. 619, and 1852, t. vii. p. 72.

II.—RESEARCHES ON THE REFLEX FACULTY.

During the last seven years I have published many papers relating to the reflex faculty.* Among the facts which I have discovered I will mention the following:

1. Grainger had found that the act of suckling can be executed by an animal deprived of its brain. I have found that even after the ablation of both the brain and the cerebellum, newly-born rabbits are able to suckle very well; which is a proof that suckling may be executed by reflex action.

2. It is commonly affirmed that the reflex power is much stronger in cold-blooded than in warm-blooded animals. This opinion is correct so far as regards the contrast between Mammals and Batrachia (the animals usually compared); but it is incorrect if Birds are compared with Reptilia and Fishes. It has been said that the higher an animal is in the scale the less it has reflex power. If this be true, we should find more and more reflex power from Mammals to Fishes; but the real order, according to my experience is: 1st, Fishes; 2d, Mammals; 3d, Amphibia and Reptilia; 4th, Birds; so that Birds have more reflex power than all the other animals, and Mammals have more than Fishes. Of course, there are exceptions to this rule in the case of particular species; thus the eel, carp and tench have as much reflex power as many Mammals possess.

It has also been commonly affirmed that the reflex power diminishes with age, being the greatest in young animals. This statement, also, has been based on a too limited induction. In Reptiles and Fishes no difference can be detected in this particular. In Birds it is decidedly the other way, the reflex power being much the strongest in adults. Among Mammals the difference is usually in favor of the young animal; not, however, at the very earliest part of its life, but ten or twelve days after birth. As to man the reflex power appears to be greater in him than in Fishes and Mammals; but it is not so energetic as in Birds and in Amphibia.

I have found that the causes of the differences between differ-

* See my Inaugural Dissertation, Paris, 3 Janvier 1846, 1ere partie.—Comptes rendus de l'Acad. des Sciences, 1847, t. xxiv. pp. 363 et 859.—Gaz. Med. de Paris, 1849, t. iv. pp. 430 et 644; et 1850 t. v. pp. 98 et 476.

ent animals, as regards the energy of their reflex power, are to be explained by anatomical differences. There exists a constant relation between the degree of the reflex power and the amount of grey matter in the spinal cord. It appears, also, that the mode of circulation of the blood in the spinal marrow has a great share in the causes of differences amongst different animals.

3. It is not necessary for the existence of the reflex power that the spinal cord should be without alteration. I have found the reflex faculty remaining in pigeons after I had crushed the spinal cord, and produced in it a considerable alteration. This is important to be known by practitioners, to prevent their drawing the conclusion, from the existence of reflex action after a fracture or a luxation of the vertebral column in man, that the spinal cord is healthy.

4. The influence of the nervous system on the secretions, by a reflex action, has been very little studied. I will state two examples of these reflex secretions: 1st, There is on the face, and particularly on the forehead and the nose, an abundant production of sweat when the nerves of the taste are strongly excited, as they are, for instance, by common salt, pepper, sugar, etc. In certain persons the quantity of sweat produced in such cases is sometimes, even in the winter, very considerable. 2d, I have observed that it is sufficient to excite the nerves of taste in order to produce the secretion of gastric juice, bile and pancreatic juice.

III.—RESEARCHES ON THE INFLUENCE OF THE NERVOUS SYSTEM UPON THE FUNCTIONS OF ORGANIC LIFE.

My experiments have convinced me that if it is certain that the nervous system is able to act, and frequently does act, on the functions of organic life, it is not the less certain that the action of the nervous system on these functions is not necessary. I hope this will be sufficiently demonstrated by the numerous facts I have to relate.

a. *Influence of the section of nerves on nutrition and secretion.*

1. The frequent occurrence of certain pathological changes after section of the sciatic nerve in Mammals, has been cited as

a proof of the dependence of the nutritive operations upon nervous agency. I think the following experiments give evidence against that doctrine. I have divided the sciatic nerve in a number of rabbits and guinea-pigs, and placed some of them at liberty in a room with a paved floor, whilst I confined others in a box, the bottom of which was thickly covered with bran, hay and old clothes. In a fortnight, the former set exhibited an obviously disordered action in the paralysed limbs; the claws were entirely lost; the extremities of the feet were swollen, and the exposed tissues were red, engorged, and covered with fleshy granulations. At the end of a month, these alterations were more decided, and necrosis had supervened in the denuded bones. On the other hand in the animals confined in the boxes, no such injuries had accrued; and although some of them have been kept living for four, five and even six months after the division of the sciatic nerve, no alteration whatever has appeared in the palsied limbs except atrophy. In these cases a portion of the nerve had been cut off, so that reunion was nearly impossible and did not take place.

Experiments made on pigeons have given the same results.

It is obvious from these experiments that the pathological changes which occur after the section of the sciatic nerve do not proceed directly from the absence of nervous action, but that they are consequent upon the friction and continual compression to which the paralysed limbs are subject, against a hard soil, owing to the inability of the animal to feel or avoid it.

In similar experiments made on frogs, I found that no alteration took place, except when water penetrated through the wound, under the skin, and between the muscles.*

2. With the help of an eminent micrographer (Dr. Lebert), I have made researches on the influences produced on the capillary circulation in consequence of the section of all the nerves (sympathetic and cerebro-spinal nerves) in the legs of a number of frogs. We have found no appearance of trouble in the capillary circulation, neither in an hour, nor in three or four days after the division of the nerves.

3. When resection of a long portion of one of the sciatic and the crural nerves is made on a very young rabbit, guinea-

* See Gaz. Méd. de Paris, 1849, t. 4, p. 880.

pig or pigeon, the palsied limb continues to grow in length, but it grows only very little, if at all, in thickness. When the experiment is made on all the nerves of the wing in a very young pigeon, it is also found that the wing grows in length, but very little in breadth or in thickness. The secretion of quills takes place equally as well in the palsied limb as in the other.

The difference in all these cases between the length of the sound and that of the palsied limb or wing is never very considerable; nevertheless the length of the healthy parts is greater than that of the paralysed parts.

4. I have found that burns, wounds and ulcerations existing in parts palsied in consequence of the section of their cerebro-spinal nerves, are cured as quickly and as well as those in sound parts.

5. Atrophy is a constant consequence of the section of the nerves of a limb. I have found that it supervenes not only in the muscles and the bones, as J. Reid has discovered, but also in the skin, which becomes evidently thinner.

6. Krimer asserts that after the section of the nerves of a limb in Mammals, the venous blood is of a bright red color like the arterial blood. (Physiologische Untersuchungen, Leipzig, 1820, p. 138 exp. 1, and p. 152 exp. 9.)

Long before the publication of Krimer, Arnemann had declared that the blood appeared darker than usual in a limb on which all the nerves had been cut. (Versuche über die Regeration an lebenden thieren, Gottingen, 1786, t. i., p. 48.)

Longet (Traité de Physiologie, Paris, 1850, t. ii., B. p. 92,) says that he has seen the venous blood retaining its ordinary color even three days after the section of the nerves of the anterior limb in dogs.

Who is right—Krimer, Arnemann or Longet? Neither of them is perfectly right. The assertion of Arnemann is entirely incorrect. By experiments made on dogs, rabbits, guinea-pigs and pigeons, I have found that the venous blood in palsied limbs is evidently less black than it is in sound limbs. But it is not true to say that venous and arterial blood in paralysed limbs have the same color. It is always very easy to distinguish one from the other.

The transformation of the arterial blood into venous is not so

complete in the palsied as in the sound limb, but it always takes place even in a great measure. There is a good proof of this in the result of my experiments on the hand and forearms of two decapitated men. I injected blood in the arteries of these parts thirteen or fourteen hours after death and when cadaveric rigidity existed. Surely there was in that case no nervous action whatever, and nevertheless the blood, which was of a bright red color when injected, came out nearly black from the veins!

From all these facts I shall conclude:

1st, That the nervous action (that of the sympathetic as well as that of the cerebro-spinal nerves) is not necessary for the change of color of the blood in the capillaries.

2d, That the nervous system of animal life has an influence upon nutrition by which it takes a share in the transformation of arterial into venous blood.

7. My friend Dr. Cl. Bernard has recently discovered the curious fact, that after the section of the sympathetic nerve in the neck, the face on the same side and more particularly the ear, become warmer and more sensible than the other side. The blood-vessels are much enlarged and a great many are visible which were not so before the operation.

I have found that the remarkable phenomena which follow the section of the cervical part of the sympathetic, are mere consequences of the paralysis and therefore of the dilatation of the bloodvessels. The blood finding a larger way than usual, arrives there in greater quantity; therefore the nutrition is more active. Now the sensibility is increased because the vital properties of the nerves are augmented when their nutrition is augmented. As to the elevation of the temperature, I have seen, as Dr. Bernard has, that the ear exhibits, sometimes, one or two degrees Fahr. more than the rectum; but it must be remarked that the temperature of the rectum is a little lower than that of the blood; and as the ear is full of blood, it is very easy to understand why it has the temperature of the blood. A great many facts prove that the degree of temperature and of sensibility of a part, is in close relation with the quantity of blood circulating in that part.

I base my opinion in part on the following experiments: If galvanism is applied to the superior portion of the sympathetic

after it has been cut in the neck, the vessels of the face and of the ear after a certain time begin to contract; their contraction increases slowly, but at last it is evident that they resume their normal condition, if they are not even smaller. Then the temperature and the sensibility diminish in the face and the ear, and they become in the palsied side the same as in the sound side.

When the galvanic current ceases to act, the vessels begin to dilate again, and all the phenomena discovered by Dr. Bernard reappear.

I conclude, that the only direct effect of the section of the cervical part of the sympathetic, is the paralysis and consequently the dilatation of the bloodvessels. Another evident conclusion is, that the cervical sympathetic send motor nerve fibres to many of the bloodvessels of the head.*

8. Nearly all physiologists believe that the secretion of the gastric juice is stopped after the section of the two pneumogastric nerves. It is difficult to solve the question by experiments on warm-blooded animals, because they die too quickly after the section of the vagi. But it is not so with frogs. I have found that they are able to live perfectly well either after the extirpation of the medulla oblongata, or after the extirpation of the ganglia of the par vagum. In both these cases I have found that digestion continues to be performed. Consequently, if the gastric juice is necessary to digestion, it is certain that this liquid is secreted.†

* My experiments prove, also, that the bloodvessels are contractile, and that the nerves are able to put them in action. I have also to remark that it is a fact, well established by Budge and Waller, that the cervical sympathetic is one of the motor nerves of the iris, and that the spinal cord is the origin of the nerve-fibres going from the sympathetic to the iris. Some experiments, which I intend to perform again, appear to prove that the same parts of the spinal cord which give origin to some of the motor nerve-fibres of the iris, originate also the motor nerve-fibres going from the cervical sympathetic to the vessels of the head. Another conclusion is to be drawn from the results obtained by Budge, Waller, Bernard and myself; it is that the cervical sympathetic, instead of receiving its fibres from upwards to give them downwards, receives them downwards and distributes them upwards.

† Comptes rendus de l'Acad. des Sciences. Paris, 1847. T. xxiv. p. 363–64.

9. J. Reid has found, that if the four nerves uniting the spinal cord to the posterior limbs are cut across on both sides, in frogs, and if a galvanic current is applied every day to the palsied limbs on one side, these galvanized limbs retain their natural dimensions, while the palsied limbs not galvanised become atrophied.

I have found:—1. That if, instead of cutting only the four cerebro-spinal nerves of the posterior limbs, I divide also the branches of the sympathetic nerve which unite with them, the same results are obtained as in Reid's experiment. 2. That if a like experiment is performed on dogs, guinea-pigs, rabbits and pigeons, the same results are found. 3. That if after atrophy has taken place in the limb of a mammal or a pigeon, a galvanic current is applied, every day, during several weeks, the atrophy diminishes little by little and the limb at length becomes as large as a sound limb. This happens although there is no return of vital property in the divided nerves. 4. That if the application of galvanism is made on the palsied limbs of very young animals, and continued every day until they have arrived at adult age, these limbs are then found to have grown as much, in every respect, as the sound limbs.

In addition to these facts I have to state that in cases of lead palsy, in which the extensor muscles, as far as I have been able to judge, were completely destroyed and replaced by fibrous tissue, I have seen muscles created by galvanism and becoming as strong as they are in healthy men.

In a case, which I published two years ago, (Gaz. Méd. de Paris, 1850, t. v. p. 169,) I have found that an increase of five centimetres in circumference took place in the superior part of the leg of a young gentleman, under the influence of galvanism, applied three quarters of an hour each day for six days.

In all the facts before related, galvanism acts by two ways: the one is that it exercises the muscles, and increase in consequence their nutrition; the other is that it produces directly some of the chemical changes which constitute nutrition.

The atrophy, which happens in paralyzed muscles, takes place mostly because they remain without exercise, and partly because when nervous action is deficient the respiration of the muscles is not carried on as well as when the nervous system acts upon

them. Galvanism applied to a palsied limb acts partly in producing the transformation of arterial into venous blood, *i. e.*, what Gustav Liebig calls the respiration of the muscles. I have seen frequently the venous blood, in palsied limbs, becoming as black as normal venous blood, after the application of galvanism. This change of coloration is not produced by a direct chemical influence, exerted by galvanism on the blood, for if galvanism is applied to blood in a vase, nothing of that kind is seen. It is in consequence of an interchange between blood and the living tissues that the change of color happens. The muscular contraction which takes place under the influence of the nervous system, or that of galvanism, produces, in both cases, an increase in the darkness of the venous blood. This fact proves that the consumption of oxygen by muscles is increased during their contraction.

I conclude from the preceding facts :—

1st. Nervous action is not necessary for nutrition.

2d. Atrophy in palsied limbs is more a consequence of absence of exercise than of any other cause.

3d. Muscular atrophy, at any stage, may be cured by galvanism.

b.—*Influence of the nervous centres on nutrition and secretion.*

1. Every one knows the singular alterations which take place in the eye after a contusion of the frontal nerve, or a section of the trigeminal or the cervical sympathetic nerves. Every one knows also that the existence of worms in the intestinal canal, and also certain affections of the spinal cord, are able to produce morbid phenomena in vision, and even diseases of the eye, and especially amaurosis. I have found that after the section of a lateral half of the spinal cord, it sometimes happens that the eye, on the same side where the cord has been wounded, will present strange and various alterations. The part of the cord having that influence on the eye, lies between the ninth and the twelfth costal vertebræ. The alteration exists generally in the cornea. In one case a ridge appeared on the anterior surface of that membrane four days after the operation. On the fifth day the ridge was deeper, and its edges had become opaque ; on the sixth day all the cornea was opaque. It remained so for

five days, after which the opacity disappeared and no trace remained of it, or of the ridge. This experiment has been made on guinea-pigs.

2. I have found a considerable hypertrophy of the two supra-renal capsules, on eight or ten guinea-pigs, upon which a lateral half of the spinal cord had been cut in the dorsal region, for eight, ten or fifteen months. These organs had acquired, in some of these cases, three times their natural dimensions, and in others only the double. There was no appearance of change in their structure.

By an examination of the supra-renal capsules in guinea-pigs, on which I had made the section of a lateral half of the spinal cord, a few hours or a few days previously, I have found these organs congested, and sometimes containing even a slight effusion of blood. It is very probable that such a congestion has been the cause of the hypertrophy found in animals operated on at a much longer time previously. The congestion is certainly the result of a peculiar disturbance in the nervous action. A part only of the spinal cord appears to possess that singular influence on the supra-renal capsules. That part is extended, in guinea-pigs, from the tenth costal vertebra to the third lumbar.

A simple puncture of the cord is frequently sufficient to produce the congestion of both supra-renal capsules.

3. The researches, made before mine, as to the influence of the spinal cord on the urinary secretion, could not give a decided result, because no physiologist had been able to keep any warm-blooded animal living a sufficient time, after the destruction of a large part of the spinal cord.

The results obtained by Ségalas on some animals who have lived from fifteen minutes to an hour after the destruction of the lumbar part of the cord, had led him to conclude that the spinal cord has no influence on the urinary secretion. Longet (Traité de Physiologie, Paris, 1850, t. ii. B. p. 199) says:—"Many observations have demonstrated to me that the visceral organs, which receive their nerves from the sympathetic, *are far from being immediately paralyzed by the section of these nerves, and that their action is even maintained much longer than the duration of the experiments in which Ségalas had destroyed the spinal*

*marrow.** Therefore I think I am allowed to maintain that after such an injury, the nerves going to these organs, and more particularly to the kidneys, do nothing but spend little by little the *nervous force*, originally and principally derived from the spinal marrow, which is the chief, if not the exclusive centre of its production; thence the persistence of the renal secretion, as well as that of the movements of the heart, the intestinal canal, the uterine cornua, etc."

I could relate a great many experiments proving the incorrectness of Longet's theory, but a single one is sufficient. I have kept living, nearly *three months*, a young cat, on which the spinal cord had been *completely destroyed* from the eleventh or twelfth costal vertebra to its termination. This cat has lived all that time in apparently good health, and its *urine* has always been perfectly normal. It was acid, as is the case constantly in cats fed on meat, milk and bread. The bladder was palsied, but the sphincter vesicæ was generally contracted, so that every day I had to compress the abdomen and the bladder to empty this pouch. When I remained two days without doing that operation, the bladder contracted in consequence of the excitation produced on its muscular fibres by their distension.

This fact clearly proves that the urinary secretion is not under the dependence of the spinal cord.

According to Krimer, the medulla oblongata is the nervous centre upon which the urinary secretion depends. My experiments prove that this opinion is incorrect:—1st. After the destruction of the medulla oblongata in frogs, I have found that the secretion of urine remains as long as the animals have lived, *i. e.*, three or four months. 2d. On hybernating mammals, in winter time I have extirpated the medulla oblongata, after having emptied the bladder. These animals have lived a little more than a day, when I took the precaution of insufflating air in their lungs many times each hour. After their death the bladder was found full of urine apparently normal.

The medulla oblongata is not therefore a centre on which the urinary secretion depends.

4. The well known opinions of Segalas, W. Philip, Krimer,

* The italics are Longet's.

Chossat, Longet and others, about the influence of the spinal cord on the functions of organic life, are quite erroneous. I have found that birds are able to live for months after the destruction of the spinal cord, from the fifth costal vertebra to its termination. This fact proves not only that the functions of organic life may continue to exist in such a case, but that they appear to be executed then as in healthy birds; for, if the operation has been made on a young bird, it will afterwards grow very well.

I have succeeded in keeping alive, from the 8th of April until the 4th of July, a young cat, about which I have already published a note in this journal.* The palsied parts in this animal have grown in length proportionately as much as the sound parts. The growth has been such in the palsied limbs that they have acquired more than double the length they had at the time of the operation. The functions of organic life appeared to exist without any apparent disturbance. The nutritive reparation was so powerful, that the pieces of the vertebral column which had been cut off have been reproduced. This fact is important, because it shows that the reproduction of bone is possible in a palsied part.

The temperature of that cat was at the ordinary degree, (105° Fahr., in the rectum.) The secretion of the hair and nails took place as in healthy cats. I had previously seen on birds that their temperature remained normal after the destruction of a great part of the spinal cord. Besides, I have found in these birds that the secretion of quills and nails continued to take place.

As to the influence of the medulla oblongata on the functions of organic life, my experiments on cold-blooded vertebrata have proved to me, that these functions (except, of course, pulmonary respiration,) may continue to exist without any appearance of disturbance.

5. After the complete transverse section of the spinal cord in mammals or birds, I have found that the ulcerations which take place around the genital organs do not result directly from the absence of nervous action. One of the causes of these ulcerations is continued pressure, and another cause is the continual presence of altered urine and fæces.

* See Med. Exam., No. v. May, 1852, p. 321.

My opinion is well proved by the following experiments:—

1st. I have put, three or four times a day and for many days, a certain quantity of urine on the posterior part of the neck, in the neighborhood of the scapulæ, upon guinea-pigs. Before a week elapsed, the skin, at the place acted on by the urine, had lost its hair and epidermis. After a week more there was an ulceration in the skin, and ten or twelve days later the skin was destroyed, and there was an ulcer with a very bad aspect. This fact proves how powerful is the action of urine on the skin.

2d. On guinea-pigs, upon which the spinal cord was cut in the dorsal region, and on pigeons, upon which the spinal cord was destroyed from the fifth costal vertebra to its termination, I have found that no ulceration appeared when I took care to prevent any part of their bodies from being in a continued state of compression, and of washing them many times a day to remove the urine and fæces.

3d. In cases where an ulceration had been produced, I have succeeded in curing it by washing and preventing compression.

4th. I have found that in animals having the spinal cord cut across, every kind of wounds or burns were cured as quickly as in healthy animals.

Therefore the ulcerations which appear, in cases of paraplegia, do not exist directly in consequence of the palsy; they can be avoided and in many cases they can be cured.

These conclusions are perfectly true in animals having had an injury to the spinal cord for a shorter time than a year; but on guinea-pigs, upon which a lateral half of the spinal cord, had been cut for fourteen, fifteen, or eighteen months, near the tenth or eleventh costal vertebra, I have found an alteration of nutrition in the palsied parts. It was the right half of the spinal cord which had been cut, and in such a case, as I have discovered, the left side of the body behind the wounded part evidently loses a portion of its sensibility, and its temperature is also diminished. I have found, at the time designated, an ulceration coming in the part between the sacrum and the hip-joint. That ulceration has taken a tolerably great extension in surface but not in depth. It became as large as a half dollar. The part ulcerated has never been subjected to any kind of compression, neither to the action of urine and fæces.

Another kind of disturbance of nutrition occurred in these animals: they lost the hair of the leg, and of the other parts in which the sensibility was diminished.

6. It is known that erection is a frequent phenomenon in men after a fracture or a luxation of the vertebral column. It is known also, that in men hanged, erection and even ejaculation are not uncommon. Ségalas says he has seen these phenomena produced by the excitation of the spinal cord. Longet (loco cit., p. 201,) declares that he has not seen the excitation of the cord producing such effects. It is very easy to ascertain, on male guinea-pigs, that Ségalas is right.

1st. A transverse section of the spinal cord always produces an erection and an ejaculation.

2d. When one of these animals is asphyxiated, erection and ejaculation take place.

3d. If the spinal cord is galvanized, erection and ejaculation are produced.

These facts prove the influence of the spinal marrow on the seminal vesicles. They empty themselves slowly when the cord is galvanized.

In asphyxia, there are universal convulsions even in the muscles of organic life, as uterus, intestine, etc. These last organs are then put in contraction, and it is not astonishing, consequently, that the seminal vesicles become also contracted. The cause of these general contractions is the excitation of the spinal cord by venous blood, and very probably by a large amount of carbonic acid, as I will elsewhere try to demonstrate.

IV.—ON THE REPARATIVE POWER OF THE NERVOUS SYSTEM.

I have recently published in this journal* the results of my researches on the reparative power of the spinal cord. From these researches I have drawn the following conclusions:—

1st. That the spinal marrow, even in adult mammalia, may be exposed to the action of the air without danger to the life of the animal.

2d. That wounds of the spinal marrow may be repaired.

* Med. Exam., No. vi., June, 1852, p. 379.

3d. That after a complete transverse section of the spinal cord, the functions of that organ may be entirely restored.

As to the nerves, the experiments of Fontana, Haighton, Tiedemann, Flourens, Steinrueck, and many others, have demonstrated the possibility of reunion of the two extremities of a cut nerve. But, in most, if not in all these experiments, the return of sensibility and of voluntary movements have not been complete. The following fact is, consequently, very important, because it proves the possibility of a complete reappearance of the lost faculties after the entire division of a nerve.*

A guinea-pig, on which the sciatic nerve had been cut across, exhibited indications of a return of sensibility a month after the operation. Two months afterwards the sensibility was increased, but was still much inferior to that of the sound limb. The muscles then were beginning to contract under the influence of the will. Six months after the section, the animal could evidently move its legs and toes voluntarily; the sensibility then was almost entirely recovered. At the end of about eleven months, the sensibility and all the voluntary movements were apparently alike in the two posterior limbs. The animal having been killed, it was found by my friend Dr. Lebert and myself that, except a slight union of muscular fibres with the nerve at the place where it had been divided, the restoration of the original condition was so complete that no indication of the division could be discovered, either with the naked eye or with the microscope. I had seen the usual swelling of the nerve at the point of reunion about the sixth month after the operation, but at the time of the last examination it had disappeared.

V.—ON TURNING AND ROLLING AS PHENOMENA PRODUCED BY INJURIES OF THE NERVOUS SYSTEM.

Pourfour du Petit and Méhée de la Touche were the first experimenters who witnessed turning produced by an injury of the nervous centres. But the first valuable researches on this phenomenon were made by Magendie and Flourens.

The parts of the cerebro-spinal centre which can be injured without producing turning, are: the cerebral hemispheres, the

* See Gaz. Med. de Paris, 1849, t. iv. p. 880.

cerebellum, the corpora striata, the corpus callosum, the spinal marrow and the olfactive and optic nerves.* All the other parts of the cerebro-spinal centres are able to produce turning or rolling.

These circulatory or rotatory movements take place sometimes on the same side, and sometimes on the side of the body opposite to that of the encephalon which has been injured.

A puncture of one of the following parts produces turning or rolling on the injured side:

1st. The anterior extremity of the thalami optici, according to Schiff.

2d. The crura cerebri, according to Magendie.

3d. The bi, or quadri-geminal tubercles, according to Flourens.

4th. The pons varolii.

5th. The posterior part of the processus cerebelli ad pontem.

6th. The auditive nerve, according to my own experiments.

7th. The medulla oblongata at the point of insertion of the facial nerve, according to my experiments in common with Dr. Martin-Magron.

8th. The medulla oblongata outside of the anterior pyramids, according to Magendie.

9th. A great part of the posterior face of the medulla oblongata, according to my experiments.

The parts of the encephalon which produce turning or rolling on the opposite side, are:

1st. The posterior extremity of the thalami optici, according to Schiff.

2d. The crura cerebri, according to Lafargue.

3d. The anterior part of the processus cerebelli ad pontem.

4th. A small part of the medulla oblongata before the nib of the calamus scriptorius and behind the corpora olivaria, according to my experiments in common with Dr. Martin-Magron.

Some of these two series of parts ordinarily produce turning and the others rolling. But these two kinds of movements can be produced by the puncture of a single part of the encephalon. Rolling is nothing but the exaggeration of turning; thus, after

* I consider as a part of the nervous centres, the three nerves of the superior senses: the olfactive, the optic and the auditive.

a puncture of the medulla oblongata, the animal at first rolls, and after some instants, instead of rolling, it turns. If, when it is turning, a slight puncture is made anew, close to the first, then the animal rolls.

Before trying to explain turning, I will give an outline of some of its species.

1st. *Turning and Rolling caused by tearing the facial nerve.*

My friend Dr. Martin-Magron and myself have discovered that if the facial nerve of a rabbit or a guinea-pig be exposed at its exit from the stylo-mastoid foramen, and be then drawn away from the cranium, so as to tear it asunder near its origin, the animal begins in about five minutes to turn itself round and round, the movement being from left to right when the nerve has been thus torn on the left side, and from right to left when it has been torn on the right side. This rotation is generally preceded by convulsive movements of the eyes, of the jaws, and of the head upon the trunk: and the body is then bent (as in pleurosthotonos) towards the injured side, by the contraction of all the longitudinal muscles of that side, the power of which is such as to resist considerable force applied to extend them. The movement at first takes place in a small circle; but the circle generally enlarges more and more, until at last, after twenty or thirty minutes, the animal walks in a straight line. There is no paralysis of any muscles, save the facial. The effect is not produced, unless the nerve be torn close to its origin.

When the nerve on the other side also is torn, even after a long interval, instead of the tendency to turn to one side, there is, at first, a rolling of the body on its longitudinal axis, which takes place towards the side last operated on. After this has continued, however, for twenty minutes or more, the animal recovers its feet, and begins to *turn*, as after the first operation, but towards the other side. This movement soon ceases.

Dr. Martin-Magron and myself think that the cause of these phenomena does not exist in the facial nerve itself, but in the part of the medulla oblongata from which this nerve has originated.*

* See Gaz. Méd. de Paris, 1849, t. 4, p. 879.

2d. *Turning and Rolling produced by an injury to the Medulla Oblongata.*

M. Magendie (Précis Elém. de Physiol. Paris, 1836, t. 1, p. 414) says: "Having raised up the cerebellum, I make a section perpendicularly to the surface of the fourth ventricle and at three or four millimetres from the median line. If I cut on the right, the animal will turn on the right side; if I cut on the left, it will turn on the left side."

If we suppose a plane cutting the medulla oblongata transversely at the distance of nearly two lines before the nib of the calamus scriptorius, the posterior face of the medulla oblongata will be divided into two parts: one before that plane, which I will call superior, and the other behind, or inferior.

Now, every puncture on that superior part produces turning or rolling on the side which has been punctured. The slightest puncture on the processus cerebelli ad medullam oblongatam is able to produce a violent and very rapid rolling. As long as the animal lives after the operation, it rolls or it turns at each time it tries to walk.

When (as Dr. Martin-Magron and myself have discovered) a deep section is made on the inferior part of the posterior face of the medulla oblongata, before the nib of the calamus scriptorius, turning is produced on the side of the body opposite to the punctured side of the medulla. A rabbit, which has lived thirteen days after the operation, had still the circulatory movement a few hours before dying. Nevertheless, sometimes the animal could walk nearly straight during a few seconds.

3d. *Turning Produced by a Puncture or a Section of the Acoustic Nerve.*

Flourens has discovered that, after the section of the semicircular canals, turning sometimes takes place.

I have found all the facts he relates about this subject perfectly right. It was interesting to know if a puncture or the section of the auditive nerve would produce turning. As it was impossible to operate on that nerve in mammals, I have experimented on frogs. In these amphibia it is easy to find the nerve and to act upon it. I have found that after a puncture or a section on the trunk of the nerve, the animal begins instantly to turn. As

long as the frogs live, after a puncture of the acoustic nerve, they turn; but the circle of turning is much smaller a short time after the operation than afterwards. I have kept such frogs for months.

4th. *On a New Mode of Turning.*

I have found a mode of turning which has altogether some of the characters of turning and of rolling.

In the circulatory movement called turning (*mouvement de manége*), the body of the animal is bent on one of the lateral sides. It has the shape of an arch, and this arch is generally a part of the circumference described by the animal when turning. The smaller the radius of that arch, the smaller is the circle of turning.

In the new mode of turning I have found, the body of the animal is not bent, and when it walks it moves laterally, instead of going forwards. In turning it describes a circle, but the longitudinal axis of its body, instead of being then a part of the circumference, is a part of a radius, so that its head is at the circumference, and its tail towards the centre of the described circle.

That mode of turning has been executed by animals on which the quadrigeminal tubercles and the pons varolii, on one side, had been punctured by a pin. One of the eyes was convulsed; the other was in its normal condition. The convulsed eye was the right one, and the tubercles punctured were those of the left side.

5th. *On the Causes of Turning and Rolling.*

I have not room enough to show that the theories of Magendie, Flourens, Henle, Lafargue and Schiff are contradicted by a great many facts. I will only present the following remarks:

1st. As the slightest puncture of certain parts of the encephalon is sufficient to produce turning or rolling, it is evident that those rotating movements do not exist in consequence of an hemiplegia, as Lafargue, Longet and Schiff believe they do. Another reason is that every degree of hemiplegia exist in man without being accompained by turning or rolling. Besides, these phenomena have been observed in persons who had no paralysis at all.

2d. The theories of Magendie and Flourens are also opposed, by the fact that a slight puncture is sufficient to produce turning or rolling.

3d. As to the theory of Henle, which is based upon the existence of convulsions in the eye, producing a kind of vertigo, it has against it the facts that, on one side, convulsions may exist in the eyes without any other disorder in the movements; and, on the other side, sometimes turning or rolling exist without any convulsion in the eyes.*

Nevertheless, I think that, in many cases, the vertigo consequent on convulsions of the eyes is one element of the cause of turning. I think also that, in certain cases, paralysis of some parts of the body may facilitate the rotatory movements. But their great cause, I think, is the existence of a convulsive contraction in some of the muscles, on one side of the body. These convulsive contractions are to be found in every case of circulatory or rotatory movement. As to the cause of these contractions, it exists in the irritation produced in certain parts of the encephalon.

VI.—ON A MEANS OF MEASURING DEGREES OF ANÆSTHESIA AND HYPERÆSTHESIA.

The curious facts discovered by E. H. Weber, on tactile sensibility, are well known. He found that if the two blunted points of a pair of compasses are applied simultaneously on the skin, there is, according to certain circumstances, either the sensation of one or of two points. When the points are both inside of certain boundaries, they are felt as one only; when they are outside of these boundaries, both are felt. These boundaries vary exceedingly in different parts of the skin, but for a given part the differences between men are not extremely considerable. I have made use of the compasses for measuring the degrees of tactile sensibility in diseases. In a case of considerable anæsthesia of the lower extremities, the patient only felt a single impression on one leg, although the

* See a very remarkable case observed by my friend Dr. Lebret, in *Comptes rendus* et Memoires de la *Soc. de Biologie* anneé 1850. Paris, 1851. t. ii. p. 7.

points of the compasses were ten, fifteen, or even twenty centimetres apart; whilst on the other leg he could distinguish them at a distance of twelve centimetres. The normal limit is generally, for that limb, from three to five centimetres. In another case where anæsthesia was slighter, the limit of the discriminating power was at from nine to sixteen centimetres. In two other cases, in which the diminution of sensibility had not been found by the other means of diagnosis, the compass indicated a very slight and beginning anæsthesia. The limit was at from six to seven centimetres.

These facts, and many others, have demonstrated to me that by the help of the compass, a physician can ascertain: 1st. Whether there is a slight anæsthesia or no. 2d. What is the degree of anæsthesia. 3d. What changes occur every day in the amount of anæsthesia.

The same is true for the cases of hyperæsthesia. In a case of paraplegia of the motor power, the patient felt the two points of the compasses, on his feet, even at the distance of five millimetres, whilst a healthy person feels the two points only when they are at a greater distance than twenty-five millimetres.

I shall add, that for succeeding in such experiments the two points must be blunted and applied simultaneously.*

(To be continued.)

Observations on Cholera in Military Practice. By W. H. Tingley, M. D., late an officer of the medical staff, U. S. Army, Member of the Academy of Natural Sciences, etc.

Of the many diseases that have claimed the attention of the medical profession, few have found so prominent a place as Epidemic Cholera. It has existed under circumstances, to all human knowledge, the most dissimilar, and exhibited an irregularity of developement and progress that has alike defied the skill of medicine, and the researches of pathology. Many hypotheses have been constructed to explain its various phenomena, but few have approximated probability, fewer still have had a semblance of truth.

* See Gaz. Med. de Paris, 1849, t. iv. p. 1012.

There are causes operating which are often coincident, yet do not produce disease; these obtain throughout the imponderable world, effecting daily and hourly changes in the vital forces; they are too subtle to be appreciated—too mysterious for human view, and into which no eye shall see, and of which no tongue shall speak. Yet if we cannot grasp the substance we may seize the shadow, and learn in time by patient observation what at first seemed insurmountable. Reason may prove fallacious, but well digested facts are 'built upon a rock.'

If the observations made by the writer, under trying and peculiar circumstances, can add aught to the already accumulated stock, he will rest satisfied that he has not labored in vain.

The command to which I was attached, consisting of three hundred and twenty men, sailed from Fort Wood, New York Harbor, April 2d, 1851, in a well and comfortably fitted transport. We arrived at New Orleans Barracks after a tedious voyage of twenty eight days, without any important sickness. From this place we embarked in a steamer for Jefferson Barracks, on the Mississippi, ten miles below Saint Louis, where we arrived in eight days, in good health. Thence we sailed in a small, dirty, and crowded steamer for Fort Leavenworth in the Indian Territory, on the Missouri, five hundred miles above its confluence with the Mississippi. On board the steamer there were many returning Californians, whose total disregard for personal cleanliness was remarkable. Four days after our departure the weather which had been mild and pleasant, became sultry, wet and warm. At this period diarrhœa appeared among the men, and two days afterwards three were attacked with unequivocal malignant cholera; on the following day two more men were taken down; of all these, two died, one in five, and the other in eight hours; these two men were the most intemperate and insurbordinate in the command. On our arrival at Fort Leavenworth the subsequent day, the command was marched to camp 'Burgwin,' three miles west of the Fort, where two hundred and fifty men had arrived, a fortnight before, from Newport Barracks, Kentucky. They had passed down the Ohio and up the Mississippi and Missouri rivers, but had no sickness among them, nor had they any at the time of our arrival, excepting the ordinary camp diarrhœa

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which attacks most soldiers when first taking the field. On the day after our arrival the cholera broke out among these men, and from this time it became rife. The site of the camp was in the basin of an undulating prairie, surrounded by alternately receding acclivities, and deeply furrowed water courses, through which the rain streamed during the heavy showers, which now fell almost hourly, into the plain below, forming pools of water in and around the company's tents and drill ground. The soil is a rich loam, based on a substratum of argillacious mould, and limestone. Several species of Nymphaceæ grow in the ponds and the Lycopodaceæ on the hill sides. The principal trees are *Juglans nigra*, *Carya olivæformis*, *Cayra alba*, *Acer saccharinum*, *Plantanus accidentalis*, *Cerasus virginianæ*, *Morus rubra*, *Quercus alba*, and *Populus canadensis*. A luxuriant growth of grass covers the lowlands, and the Silphium lancinatum (Pilot weed) abounds. The prevailing winds were from south and southeast. The average temperature, for one week was (in round numbers) 72° Fahr. during the day, and 56° during the night. The greatest daily extremes were 83° and 58°. The greatest nightly extremes, 56° and 50°. The weekly average of Hygrometric observations, showed the 'dew point' at 66°, its greatest extremes 70° and 63°. The hygrometic observations were made at daylight, and 3 P. M., (according to Kœmptz, the relative humidity of the atmosphere is greatest at daylight.) The weekly average of the barometer was 29.231, its greatest extremes 29.854 and 28.105.

The tension of the atmospheric electricity was considerably diminished; this condition is nearly always coincident with a high 'dew point.' The electricity of the atmosphere, like the barometer, undergoes several *horary* variations. According to Becquerel, its least intensity is at daylight, and 3 P. M. At these respective hours the majority of cholera cases were reported, more, however, at the former than the latter hour. The sound of the reveille drum frequently brought the hospital steward to my tent to say, 'another man just taken down.' The disease began to decline as soon as the weather became settled—when the relative humidity of the air was diminished, and when we had crossed the valley of the Kansas river, and began the ascent to the upland 'plains.'

According to Cavallo, the atmospheric electricity is weakest in hot weather, and previous to a shower of rain. The observations of Mr. Howard have proved—first, that the positive electricity common to fair weather disappears and yields to a negative state before rain. Secondly, that the rain that falls first after a depression of the barometer is negative. To these, I can add my own observations. The night on which the disease was most rife, was the most terrific I have ever witnessed. The rain fell in torrents, swelling the stream near our camp, which we had crossed nearly dry shod a few hours previous, to its highest banks. The fitful blasts howled through the thick foliage of the forests trees, overturning in its resistless course many tents, and exposing their inmates to its merciless rage. The lightning played with incessant glare, and the thunder roaring far and near, mingled mournfully harsh with the shrieks and agonising cries of the dying. On that night the spirit of pestilence and the spirit of the storm stalked forth arm in arm.

Dr. Crawford, (Observations on the Asiatic Cholera in St. Petersburg,) says: "During the latter end of May, and the whole of June, a remarkable change took place in the weather. There were almost constant high winds, shifting frequently and suddenly round to every point of the compass, and often accompanied by torrents of rain, and sometimes thunder. This disturbed state of the atmosphere was indicated by sudden fallings and risings of the barometer, sometimes to the extent of between one and two inches. The changes of the temperature were equally frequent and rapid, the heat being for several days very great, as high as 84° to 90° Fahr., and the air exceedingly sultry and oppressive, and a damp, relaxing south wind; and then suddenly on a change of wind, and sometimes on the occurrence of a thunder storm, this oppressive heat would be succeeded by great cold, the thermometer falling as much as 50° in a few hours, so that it was in June several times below the freezing point. Another peculiarity in the condition of the air, was the disturbed state of its electricity. This was clearly demonstrated by the fact, that the electric machines could not be charged, and to a great extent lost their power, which generally happens whenever the atmosphere is damp and unsettled." Muller takes a somewhat opposite view. (*Einige Bemerkungen über die Asia-*

tische Cholera.) He says, "Throughout the whole duration of the epidemic, the air was oppressive, changeable, and heavy. Thunder storms occurred often, but had no influence on the number of attacks or recoveries. Rain fell almost daily, the heavens were gloomy, the evenings foggy, and the sun was seldom visible." Dr. Muller does not say whether he kept a daily record of the state of the atmospheric electricity. When in China, several years since, I was informed by an officer of the British army, that before and during the malignant fever which ravaged the island of Hong Kong the year previous to my visit, 1843, the weather was variable, alternately hot and cold with heavy rains, and that there was no 'electricity in the air.'

I have been informed by my friend Lieutenant Sturgis, 1st. U. S. Dragoons, that a detachment of men under his command which started from New York in May, 1850, for Fort Leavenworth, via the Lakes, had no sickness until they arrived at Lasalle, Illinois, after they had been for several days crowded in a small and dirty canal boat. On their arrival at the aforesaid place, the cholera broke out among them and continued with the command, until they had crossed the valley of the Kansas river. Although there was no meteorological register kept, Lieut. S. says, 'the weather was hot and rainy during the day, and cold at night.'

The principal conditions under which the electricity of the air is developed, require but a passing notice. They are friction of the air against the earth. Change of state in fluids—evaporation; it is evolved during the germination of plants, and the breaking of crystals.

My experience strongly inclines me to seek for the causes of cholera, and many, if not all, diseases of a pestilential nature, in the irregularities and diminished tension of the atmospheric electricity. The agency of these changes in producing a predisposition to disease may be considered in a three fold relation. First, as acting, *per se*, separate and apart from all correlative conditions. Second, as influencing the condition of the efficient causes, whether vegetable, animal, geological, or fungous in their nature, and predisposing the body to the operation of one or more of these. Third, as acting in combination with one more of the above. In the barren and uncertain state of statistical know-

ledge of the effects of physical agents on the animal constitution, the difficulty of selecting one of these conditions above the rest will be apparent. The first proposition may with propriety be rejected, because the laws of nature are immutable, and when determined we have a *point depart* for judging of analogous states.

The laws which regulate our sphere, and the thousands of worlds surrounding it, have been in operation since 'time was.'

'Such as creation's dawn beheld
Behold thee now.'

One drop of water less in the world than when it was created, would cause an eccentricity in its motion, whose consequences might prove a 'wreck of matter.'

The compass that guides the mariner, has its own fixed principles. Light has its own laws. If these were changing, excepting according to known and regular progression, the interests of commerce, of navigation, and of science would be forever destroyed. The barometer in its mutations is faithful and constant, predicting to a certainty the coming tempest, and enabling the mariner to protect his ship and his life against the 'strife of elemental war.' Why should not electricity, in its relation to organized matter, have its fixed laws likewise? No one can doubt that the identical state of the atmospheric electricity which at one time produces pestilence, at another time may be compatible with the most vigorous health. The choice therefore lies, necessarily, between the second and third propositions. Here the difficulty is increased tenfold. I shall, however, select the second, because under the circumstances of the first appearance of the disease among our men, there was sufficient cause for the developement of a pestilence that had for several days foreshadowed its invasion. If we assume that the active cause was of a vegetable character, we certainly had enough decaying vegetable matter and filth to prove a *foyer* of disease. If it is admitted that it is animal in its nature, there is a ready explanation in the crowding together of so many men in a narrow and imperfectly ventilated space, and whose personal cleanliness was totally disregarded. If the fungous theory is viewed as causing a proclivity to pestilential disease, we might have found conditions amply efficient. Furthermore, opposed to the last proposition, which

presupposes immediate action under concurrent conditions of air and earth, is the fact that the men complained of cramps in the upper and lower extremities, of lassitude, debility and vertigo, which is sufficient to prove the action of this subtle agent on the human frame. Dr. Holland, (Medical Notes) says: "One of the best tests of the actual operation of the atmospheric electricity on the body is, as I think, that mixed sensation of heat and cold which most persons must recollect at some time to have felt—or rather the consciousness of sensations which cannot clearly be defined to be either. Concurrently with such a state of atmosphere which the thermometer does not in any way interpret to us, there generally occurs more or less of the lassitude before described, the muscles are readily fatigued, some degree of headache is often felt, and other vague uneasiness of the bodily feelings, varying much in different habits, and doubtless influenced by the condition of the health at the time."

I believe that the subtle agent (electricity) primarily affects the nervous system through the medium of the lungs, and that the blood disorder is a consequence of irregular innervation. We see this in the diminished power of calorification, and the depressed and disordered circulation. It is under such conditions that efficient causes may require a force and intensity that under a positively electrified state of atmosphere they do not possess. The *prodromes* were cramps, lassitude, debility and vertigo; the biliary secretion was suppressed, and the portal circulation congested, as evidenced in the clay colored stools, discharges of blood from hemorrhoidal tumors, bitter pasty taste in the mouth, as if the patient had 'eaten a bad egg,' or 'drunk bilge water,' alternately constipation and diarrhœa. If we admit that the impression of the morbific agent is made on the nervous system, and, following this, on the blood, we can readily account for the hæmatological changes and the irregular transmission of nervous power, so well marked in cholera.

Those who have witnessed the enormous discharges from the intestinal mucous membrane, and the suddenness with which the cholera often makes its invasion, will not doubt a two fold operation of the efficient cause or causes. To those who doubt it, and view the nervous and muscular symptoms as sequacions to primary action in the blood, I would only ask, if these should not

show marked effects only after changes noticed to the blood—should not blood drawn from a vein during the *Prodromes*, show some physical or chemical change. The former I have never observed; the latter I had not the means of observing.

Prevost found that an electric spark passed through a drop of blood, caused the particles it contained to partially separate from their elementary globules.

The speedy course of the disease is sometimes remarkable. Twice I have seen cavalry soldiers, apparently in good health, fall from their horses and die within an hour. Two men who died thus suddenly, I know were and had been in good health for ten or twelve days. With the exception of hydrocyanic acid there is no agent, save electricity, that can so entirely disorganize the blood, and this agent must first produce its effect on the great nervous centres.

The question of the propagation of cholera by contagion has claimed no inconsiderable attention. Many singular facts have been adduced by the contagionists and non-contagionists. Surgeon Thomas Lawson, now Surgeon General, in his official report, 1832, says: "No case of the disease manifested itself at New Orleans until the arrival in port of the steamer 'Constitution,' which had several cases on board—a number of the passengers having already fallen victims to the disease; so fearfully rapid was the pestilence in its progress, than in less than forty-eight hours it reached the lowest plantation on the Mississippi, desolating almost every spot inhabited by man. Like a skilful general, it seems to have advanced upon the capital, leaving the minor posts on the line of march untouched. Whatever may have been the cause of the disease, it was unknown among us until the steamer Constitution arrived in port."

There had been no cholera in any of the towns on the Mississippi river, and it was not until several days after our arrival at Fort Leavenworth, that it appeared in Independence, Missouri. Nor had there been any cases at the Fort. For two months the disease lingered about the river towns, and nearly every steamer passing up and down the river had more or less of the pestilence on board. On our march we overtook a train of Santa Fe traders who, up to this time, had been in excellent health; after this the disease broke out among them, but not with great malignity.

Many of the hospital orderlies were attacked while attending the sick. At a Pottawattomie Indian settlement near which we passed, the disease raged for a week with fearful mortality. Assistant Surgeon Kennedy and myself were attacked nearly at the same time, without any premonition, while attending to our professional duties. He died in six hours; I suffered a long and tedious convalescence. Company B., First U. S. Dragoons, passed over our route a fortnight after us, and had the same inclement weather, yet not one man was reported sick; but they were old and acclimated soldiers, well used to the field. From these and similar facts in the practice of other medical officers, I was induced to believe that cholera is generally *contagious*, but that under favorable circumstances it may be *non-contagious*.

In treating this disease two methods were employed—the sedative, and the stimulant and alterative. The first consisting of opium and acetate of lead, was adopted by my colleague, Assistant Surgeon Kennedy. The last method was followed by myself. The combination was calomel in large proportions, (I have found this drug in large doses, xx. grs., relieve the nausea and irritability of stomach), camphor, capsicum and quinine, and small quantities of acetate of morphia. In a few cases, I gave calomel combined with rhubarb and ipecacuanha; in civil practice this treatment may be beneficial in some cases, but soldiers and sailors require more heroic medication. Those cases which terminated fatally, treated by the first method, usually died during the stage of collapse; those who died after the treatment by the second method died either during the stage of reaction, or from some intercurrent or consecutive disease, as apoplexy, typhoid fever, pneumonia, or gastro-enteritis. When I saw a patient before the *algide* state commenced, the comparative ratio of deaths was as 42 to 54. Those who have not witnessed the destitute and comfortless condition of troops on a march, cannot readily understand how mortality can be so great during an epidemic. Within a few days, I have heard from a detachment of troops which left in May for Santa Fe. They had had the cholera, and the mortality was one out of every eight men composing the command. I believe this treatment would be more successful in private practice, where such valuable adjuvants as warm and vapor baths can be obtained, to say nothing

of the advantage of houses over tents for invalids. After a trial of several methods of treatment, I am satisfied with the superiority of the stimulant and alterative treatment above detailed. From what I have seen of the opium treatment, I am not disposed to advocate it. When it is beneficial it is in the very early stages of the disease, when there has been precursory diarrhœa, and before the characteristic rice water discharges appear. I have seen the error of 'locking up the bowels,' and retaining the morbid secretions of the intestinal canal in contact with its mucous membrane. Moreover, I have seen patients *overdrugged* more than once by not very large doses of this medicine. The opium treatment is emphatically empirical—it combats symptoms, not the disease. When the nature of the disease is changed by calomel, quinine, &c., or by a "crisis'—as soon as the alvine evacuations become fœcal and bilious, the mineral astringents (acetate of lead I prefer) exert a salutary and tonic influence on the gastro-intestinal membrane. I have tried several of the vegetable astringents, but have no faith in them.

In a few cases I bled, not empirically, but to relieve cerebral congestion—this was never urgent save in the reactive stage, when the injected conjunctiva, pain across the temples, heaviness of intellect, and partial coma, indicated the condition of the cerebral circulation. Spasmodic action of the muscles, and cramp were relieved by sinapisms, and towels wrung out of hot vinegar. I have tried chloroform, and have the fullest reasons for being dissatisfied with it. Sometimes cholera assumes anomalous forms. An officer of the staff was attacked and went through the disease without having more than five or six evacuations per *anum*; he however, ejected per *orem*, matter in all respects resembling 'black vomit,' and was so near death that he had involuntary alvine discharges, a state usually considered beyond hope. He suffered severely for several weeks with gastralgia, cardialgia, and a paralytic condition of the lower portion of the colon. I prescribed friction with dry mustard to the abdomen, diluted tincture of cantharides to the spinal column, small doses of brandy and the alcoholic extract of nux vomica, until urethral irritation was induced; under this treatment he gradually recovered.

I made but few necroscopic examinations, and those, made after the fatigues of a march, were necessarily hasty. In those

who died during the stage of collapse, the mucous membrane of the stomach and intestinal canal was pale, soft, and easily detached from the subjacent tissue. The liver was anæmic, and the gall bladder filled with thick bile. In those who died during the stage of reaction, there were traces of *diptheritic* inflammation, enlargement of the mucous follicles, and engorgment of the mesenteric veins. In one case there was found a state of red hepatization of the lungs; this condition was suspected during life. My attention was first directed to it by the comatose condition of the patient, which evidently did not arise from the disease, (cholera). The engorged upper lobes pressed upon and obstructed the blood in the superior vena cava.

There has not been much written respecting the *sequelæ* of cholera; some have considered abdominal typhus as one; others a well developed *gastro-enteritis* as another. My own observations do not coincide with either of these views. I have never seen the tenderness about the right iliac region which marks the first, nor the pain and tenderness about the umbilicus, so characteristic of the other; but I have seen an accelerated pulse, dry, harsh skin, furred tongue, morbidly increased appetite and diarrhœa, which are prominent symptoms of a low form of intestinal inflammation; and this condition I consider the true consecutive disease. In a few cases the affection of the duodenum extends to the biliary ducts, and to the stomach, but the gastric disorder is strictly functional, and consists in disordered secretion of the gastri follicles, and atony of the muscular coat. The disordered stomach is soon relieved, but the intestinal disease is present even after many years. The brain is more or less affected—dimness of vision, vertigo, and mental inaptitude show the disordered innervation of this "great central power."

I have used with benefit, in this condition, blisters over the abdomen, mercury, ipecac, and taraxacum combined, with enemeta of tepid water twice daily. Where it can be obtained, the warm shower bath is a valuable adjuvant. When all symptoms of inflammatory action have subsided, and the bowels are left in an atonic condition with large accumulation of gas within them, there is no medicine that can compare with the nux vomica. I prefer the alcoholic extract to any other preparation; under its use I have seen the most distressing symptoms speedily relieved.

Mortality of Philadelphia, for April, May and June, 1852, *arranged from the Record kept at the Health Office. With remarks on the new "Registration Law."* By WILSON JEWELL, M. D.

The second quarter of the present year, commenced on the 28th day of March, and terminated with the 3d of July, ult., embracing a period of 14 weeks and 1 day, or 99 days. According to the returns found at the Health Office, the number of deaths during the quarter, from all causes, amounted to 2634, showing a falling off of 151, or 6 per cent., from those of the first quarter of the year.

TABLE No. 1 Exhibits a classification of the various causes of death during the quarter, together with an enumeration of the sexes, as well as the number of deaths for each month separately.

Comparing this table, with the same one for the first quarter of the year, as published in the June No. of this work, it will be found that the principal falling off has taken place among the deaths from diseases of the organs of respiration, amounting to 164, or 22 per cent.

TABLE No. 2 Furnishes an accurate account of the names of the diseases found on the Register at the Health Office, being a faithful transcript from the certificates of the physicians. This nomenclature, which at the best is an imperfect one, we have not disturbed, preferring to allow each physician to speak for himself, rather than alter his phraseology, in order to show, how difficult it is to compile a statistical abstract, which would approach towards accuracy, in making comparisons of the prevalence of different diseases at distinct periods of the year, while this want of uniformity in attaching names to diseases shall continue to be observed.

This table also includes the number of deaths from the various causes assigned therefore, at fifteen distinct periods of life.

The deaths from Consumption of the Lungs as usual, occupy a large space in the table, amounting to 324; 175 of which took place between the ages of 20 and 24. The deaths from Consumption for the past six months have exceeded those for the same period of 1851, by 221—to wit:

	1851.	1852.	
1st and 2d quarter,	461	682	Excess of 1852, 221.

Of the deaths recorded under the head of diseases of the Nervous System, amounting to 469, 315 took place in children under five years of age. The deaths from Convulsions alone, amounted to 131; 68, or more than half, occurring in children under one year of age.

The deaths from Scarlet Fever amounting to 98, have fallen off since the last quarter, about 35 per cent.

The mortality from Typhus and Typhoid Fevers has more than doubled itself since our last report. During the first quarter, January, February, and March, the deaths from these fevers numbered 48. The quarter just ended, shows 91 deaths, being an increase of 43.

Small Pox and Varioloid still continue to prevail. The deaths for this quarter number 137, two-thirds of which were under ten years of age, a striking evidence of the want of attention to vaccination among certain classes of our population. The deaths from Small Pox for the last six months, have amounted to 402; equal to two deaths per day.

By a reference to the tables in the June No. of the Examiner, it will be found that the deaths from Dysentery have doubled themselves. During the first quarter of the year, there were 34; while the second gives 75.

The deaths from Cholera Infantum have also increased during the present quarter. 44 cases of deaths are recorded, 40 of which were under two years of age.

TABLE No. 3 Presents the aggregate number of deaths during the second quarter, from all causes, for fifteen distinct periods of life. By this analysis, it will be seen that nearly half the deaths that have been registered, occurred in children under five years of age. Exclusive of the "Still Born," 551, or nearly one fourth were under one year of age.

Beyond the fifth year of life, the highest number of deaths took place between the ages of 30 and 40.

The deaths from "Still Born," 127; "External Causes," 72; "Old Age," 61; "Debility," 71, and "Unknown," 81, in all 412, will leave 2222 deaths from known diseases, which is equal to 22½ deaths per day, during the quarter.

TABLE NO. 1

Deaths for the second quarter of 1852, *classified.*

	April	May.	June	Male	Fem.	Total.
1. *Endemic and Contagious Diseases.* Zymotic or Epidemic,	235	175	213	309	314	623
2. *Uncertain or general seat.* Sporadic Diseases	108	87	120	173	142	315
3. Of the Nervous System,	180	132	157	269	200	469
4. Organs of Respiration,	248	190	146	314	270	584
5. " Circulation,	28	16	26	34	36	70
6. Of the Digestive Organs,	59	58	57	77	97	174
7. " Urinary "	4	3	7	11	3	14
8. " Organs of Generation,	14	7	3		24	24
9. " Organs of Locomotion,	3	6	5	7	7	14
10. " Integumentary System,		5	1	4	2	6
11. Old Age,	25	18	18	20	41	61
12. Of External causes,	19	21	32	55	17	72
Still Born,	53	26	48	70	57	127
Unknown,	23	25	33	39	42	81
	999	769	866	1382	1252	2634

TABLE No 2.

1. *Endemic and Contagious Diseases.—Zymotic or Epidemic.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Aphthæ	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
Cholera Infantum	21	20	30	10	3	1	0	0	0	0	0	0	0	0	0			44
" Morbus	5	1	1	1	1	0	0	0	0	2	1	0	0	0	0			6
Croup	23	25	5	14	24	5	0	0	0	0	0	0	0	0	0			48
Diarrhœa	14	11	13	2	0	1	0	1	1	0	2	3	1	0	1			25
Dysentery	39	36	10	15	16	3	1	0	6	4	6	5	5	3	1			75
Erysipelas	8	16	7	1	2	0	0	0	0	3	4	3	2	2	0			24
Fever	5	4	2	0	1	0	1	0	0	2	1	1	1	0	0			9
" Congestive	3	0	0	0	2	1	0	0	0	0	0	0	0	0	0			3
" Hectic	0	2	0	0	0	1	0	0	0	1	0	0	0	0	0			2
" Inflammatory	0	1	0	0	0	0	0	0	0	1	0	0	0	0	0			1
" Irritative	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
" Intermittent	1	0	0	0	0	0	0	0	0	1	0	0	0	0	0			1
" Remittent	5	5	0	1	2	2	0	0	0	0	0	1	4	0	0			10
" Scarlet	40	58	7	17	41	31	0	0	1	1	0	0	0	0	0			98
" Synocha	1	0	0	0	0	1	0	0	0	0	0	0	0	0	0			1
" Typhus	36	21	1	1	0	1	2	2	15	10	13	8	4	0	0			57
" Typhoid	18	16	0	0	0	3	0	2	9	11	3	1	5	0	0			34
Hooping Cough	0	2	1	0	1	0	0	0	0	0	0	0	0	0	0			2
Measles	16	27	11	19	12	1	0	0	0	0	0	0	0	0	0			43
Small Pox	67	67	22	17	36	15	4	4	23	10	1	1	0	1	0			134
Syphilis	0	1	0	0	0	0	0	1	0	0	0	0	0	0	0			1
Varioloid	2	1	2	0	0	1	0	0	0	0	0	0	0	0	0			3
	309	314	114	98	141	67	8	10	55	40	31	23	22	6	2	0	0	623

2. *Uncertain or General Seat.—Sporadic Diseases.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess	2	2	0	1	0	0	0	0	1	0	2	0	0	0	0			4
" of Neck	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0			1
" of Thigh	1	0	0	0	0	0	0	1	0	0	0	0	0	0	0			1
Amputation of Leg	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0			1
Cancer	2	3	0	0	0	0	0	0	1	1	1	2	0	0				5
Cachexia	6	0	3	0	0	1	0	0	1	0	0	1	0	0	0			6
Congestion	0	1	0	1	0	0	0	0	0	0	0	0	0	0	0			1
Cyanosis	3	7	8	0	1	0	0	1	0	0	0	0	0	0	0			10
Cancer of Breast	0	3	0	0	0	0	0	0	0	2	0	1	0	0	0			3
Debility	36	35	33	2	2	1	0	2	1	6	9	8	5	1	1			71
Disease of Throat	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0			1
Dropsy	20	16	0	1	5	4	2	2	0	4	2	7	3	5	1			36
Effusion	1	0	0	0	1	0	0	0	0	0	0	0	0	0	0			1
Gangrene	2	1	0	0	0	1	0	0	0	0	0	2	0	0	0			3
" of Mouth	0	1	0	0	1	0	0	0	0	0	0	0	0	0	0			1
Hemorrhage	3	0	0	0	2	0	0	0	0	0	1	0	0	0	0			3
" from Umbilicus	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0			2
Inanition	14	11	23	0	1	0	0	0	0	0	0	0	0	0	1			25
Inflammation	0	2	1	0	0	0	0	0	1	0	0	0	0	0	0			2
" Glandular	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0			2
Injury of Perineum	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0			1
Malformation	5	1	6	0	0	0	0	0	0	0	0	0	0	0	0			6
" of Rectum	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
Marasmus	45	41	48	17	9	0	1	2	2	0	0	2	2	2	1			86
Melœna	1	1	0	0	0	0	0	0	0	1	0	0	1	0	0			2
Mortification	2	0	0	0	0	0	0	0	0	0	0	0	0	1	1			2
Scirrhus of Breast	0	1	0	0	0	0	0	0	0	0	0	1	0	0	0			1
Scrofula	10	12	5	3	6	1	1	2	0	2	2	0	0	0	0			22
Tabes Mesenterica	11	2	5	5	2	0	0	0	0	1	0	0	0	0	0			13
Tumour of Neck	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0			1
Ulceration	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
	173	142	136	32	30	8	4	10	6	17	20	25	13	9	5	0	0	315

3. *Of the Nervous System.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Ædema of Brain	1	0	0	0	0	0	0	1	0	0	0	0	0	0				1
Atrophy "	0	1	1	0	0	0	0	0	0	0	0	0	0	0				1
Apoplexy	12	11	0	0	0	0	0	1	1	6	5	2	6	0	1	1		23
Asthma Thymic	1	0	0	1	0	0	0	0	0	0	0	0	0	0				1
Cerebral Fever	1	0	0	0	0	0	0	0	0	1	0	0	0	0				1
Concussion of Brain	0	1	0	0	0	0	0	0	0	0	1	0	0	0				1
Congestion "	25	20	8	7	4	3	0	1	7	5	6	0	1	2	1			45
Compression "	0	1	0	0	0	0	0	0	1	0	0	0	0	0				1
Coma	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0			1
Convulsions	71	60	68	29	21	5	2	1	1	1	2	1	0	0	0			131
" Puerperal	0	5	0	0	0	0	0	2	2	1	0	0	0	0	0			5
Cramp	2	1	1	0	1	0	0	0	0	1	0	0	0	0	0			3
Coup de Soliel	3	0	0	0	0	0	0	0	2	1	0	0	0	0	0			3
Disease of Brain	15	9	11	5	1	3	1	0	0	1	0	1	1	0	0			24
Dropsy "	42	40	32	21	21	4	0	0	1	0	0	0	0	0	0			82
Epilepsy	8	0	0	0	0	0	1	1	1	2	3	0	0	0	0			8
Effusion of Brain	16	12	3	13	4	0	1	0	0	1	2	2	2	0	0			28
Inflammation of Brain	47	25	23	15	11	8	1	3	4	1	0	2	0	1	0			72
Injury "	0	1	0	0	0	0	0	0	0	1	0	0	0	0	0			1
Insanity	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0			1
Mania	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0			1
Meningitis	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
Mania a Potu	13	2	0	0	0	0	0	0	1	5	3	4	1	1	0			15
Neurosis	0	1	0	0	0	0	0	0	0	0	0	1	0	0	0			1
Palsy	4	8	0	0	0	0	0	0	0	1	3	1	3	3	1			12
Spasm of Glottis	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
Softening of Brain	0	1	0	0	0	0	0	0	0	1	0	0	0	0	0			1
Tetanus	1	0	0	0	0	0	1	0	0	0	0	0	0	0	0			1
Trismus	2	0	1	0	0	0	1	0	0	0	0	0	0	0	0			2
Tuberculosis Cerebri	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0			1
	269	200	151	94	66	23	8	10	21	29	26	15	14	8	3	1	0	469

4. *Organs of Respiration.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess of Lungs		2										1		1				2
Asthma	2	1		1							1				1			3
Congestion of Lungs	8	5	7	2	1	1	1			1								13
Consumption "	165	159	7	4	7	4	11	25	84	91	45	32	9	3	1	1		324
Coryza	1		1															1
Disease of Lungs	3	3	1				1				1	1	1		1			6
" Chest	1									1								1
Dropsy of Chest	11	11	1	2	5	2	1	1	1	1	2	2	1	2	1			22
Effusion "	3										1	2						3
" Lungs	1		1															1
Gangrene "	1							1										1
Hemorrhage "	3	2						1			3		1					5
Inflammation "	70	52	29	22	18		2		11	8	10	4	14	2	2			122
" Bronchia	34	21	20	14	7	4			3	1	2		3	2	2			58
" Chest		1	1															1
" Larynx	3	6	1	3	1		1			1		1	1					9
" Pleura	2	1						1	2									3
" Throat	2	1	1	1	1													3
" Tonsils	1				1													1
Palsy of Lungs	1										1							1
Tuberculosis	2	2	1	1			1			1								4
	314	270	71	50	41	11	18	29	101	105	66	43	30	10	8	1	0	784

5. *Organs of Circulation.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Anæmia	2	2	2							1		1						4
Angina Pectoris		1											1					1
Disease of Heart	22	19	2		1	4	3	1	4	5	7	3	6	4	1			41
Dropsy "	2	2				1			1				2					4
Enlargement "	3	2	1					1		1	1	1						5
Inflammation of Heart	3	5	1		2				4	1								8
" of Vein	1							1										1
Malformation of Heart		1	1															1
Ossification "		1												1				1
" of Arteries	1														1			1
Rheumatism of Heart		3					1			2								3
	34	36	7	0	3	5	4	3	9	10	8	5	9	5	2	0	0	70

6. *Digestive Organs.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Abscess of Liver	0	1	0	0	0	0	0	0	1	0	0	0	0	0	0	0		1
Abdominal Dropsy	5	6	0	0	0	1	0	0	0	0	3	3	2	1	1	0		11
Cancer, Bowels	0	1	0	0	0	0	0	0	0	0	1	0	0	0	0	0		1
" Liver	0	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0		1
" Rectum	1	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0		1
" Stomach	0	3	0	0	0	0	0	0	0	0	1	0	2	0	0	0		3
Cancrum Oris	2	4	1	1	2	2	0	0	0	0	0	0	0	0	0	0		6
Cirrhosus of Liver	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0		1
Congestion "	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0		1
Colic	2	2	2	0	0	0	0	0	0	0	1	0	0	1	0	0		4
Consumption of Bowels	0	1	0	0	0	0	0	0	0	0	1	0	0	0	0	0		1
Disease "	0	2	0	0	0	0	0	0	0	0	1	0	1	0	0	0		2
" Liver	3	1	0	0	0	0	0	0	1	0	0	1	0	2	0	0		4
" Stomach	3	0	0	0	0	0	0	0	0	1	0	0	1	1	0	0		3
Dropsy of Liver	0	1	0	0	0	0	0	0	0	0	0	0	0	0	1	0		1
Enlargem't "	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0		1
" Spleen	0	1	0	0	0	0	0	0	0	0	1	0	0	0	0	0		1
Gastric Fever	0	1	0	0	0	0	0	0	1	0	0	0	0	0	0	0		1
Hemorrhage of Bowels	1	1	0	0	0	0	0	1	1	0	0	0	0	0	0	0		2
Hernia	2	1	0	0	0	0	0	0	0	0	0	2	1	0	0	0		3
Ileus	0	1	0	0	0	0	0	0	0	1	0	0	0	0	0	0		1
Indigestion	1	1	1	0	0	0	0	0	0	0	0	0	0	0	1	0		2
Inflam. Stomach & Bowels	22	30	11	6	3	0	0	1	5	6	3	4	4	7	2	0		52
" Liver	7	2	2	0	0	1	0	0	0	0	0	2	2	1	1	0		9
" Peritoneum	9	16	0	0	1	1	1	4	6	8	2	2	0	0	0	0		25
Intussusception	2	1	2	0	1	0	0	0	0	0	0	0	0	0	0	0		3
Jaundice	2	3	3	0	0	0	0	0	0	1	0	1	0	0	0	0		5
Mortification Bowels	1	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0		1
Obstruction Bowels	1	2	1	0	0	0	0	0	0	1	0	1	0	0	0	0		3
Perforation "	2	0	0	1	0	0	0	0	0	0	1	0	0	0	0	0		2
Scirrhus Pancreas	1	1	0	0	1	0	0	0	0	0	0	1	0	0	0	0		2
" Pylorus	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0		1
" Stomach	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0		1
Softening Stomach	1	1	1	1	0	0	0	0	0	0	0	0	0	0	0	0		2
Teething	2	2	1	3	0	0	0	0	0	0	0	0	0	0	0	0		4
Ulceration of Bowels	3	3	0	0	0	0	0	1	1	1	1	1	0	1	0	0		6
" Stomach	1	1	0	0	0	0	0	0	0	1	0	0	0	1	0	0		2
" Throat	0	3	0	1	2	0	0	0	0	0	0	0	0	0	0	0		3
Worms	1	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0		1
	77	97	26	13	12	5	1	7	16	21	18	19	13	19	4	0	0	174

7. *The Urinary Organs.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Albuminuria . .	1											1						1
Diabetes . . .	2									1	1							2
Disease of Kidneys .	2	2			1	1			1		1							4
Inflam. of Bladder .	1	1				1					1							2
" Kidneys .	4										2	1			1			4
Stone	1													1				1
	11	3	0	0	1	2	0	0	1	1	5	2	0	1	1	0	0	14

8. *Organs of Generation.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Cancer Uterus . .		3									1	1			1			3
Chlorosis . . .		1						1										1
Hemorrhage from Uterus		2								2								2
Inflammation "		6								6								6
Puerperal Fever . .		8							5	1	2							8
" Mania . .		1							1									1
Ovarian Dropsy . .		1							1									1
" Tumor . .		2											1	1				2
	0	24	0	0	0	0	0	1	7	9	3	1	1	1	1	0	0	24

9. *Organs of Locomotion.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Caries of Cranium .	1									1								1
" Jaw . .		1										1						1
" Spine . .	1											1						1
Disease of Spine . .	2	1			2	1												3
Effusion of Spinal Marrow		1								1								1
Medul. Tumor Knee joint		1							1									1
Rheumatism . .	3	2					1		1	1	1	1						5
Spinal Meningitis .		1			1													1
	7	7	0	0	3	1	1	0	2	3	1	3	0	0	0	0	0	14

10. *The Integumentary System.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Purpura . . .	1	1	2															2
Scorbutus . . .	2								2									2
Suppressed Eruption .	1				1													1
Ulcers . . .		1												1				1
	4	2	2	0	1	0	0	0	2	0	0	0	0	1	0	0	0	6

11. *Old Age.*

Old Age . . .	20	41											6	28	24	3		61

12. *From External Causes.*

	Male.	Female.	Under 1 yr.	1 to 2.	2 to 5.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	100 to 110.	Total.
Asphyxia . . .	3	3	6															6
Burns and Scalds . .	3	2		3	1	1												5
Casualties . . .	13	2	1	1		2	1	1	6	2		1						15
Drowned . . .	24	3			1	2	1	2	6	9	6		1					27
Exposure . . .	1												1					1
Fracture Leg . .	1								1									1
" Pelvis . .	1								1									1
" Skull . .	1									1								1
Gun-shot Wound . .	1								1									1
Intemperance . .	2	4							1	4	1							6
Suffocation . . .	1	3	3									1						4
Suicide . . .	3								1		2							3
Wound . . .	1								1									1
	55	17	10	4	2	5	2	3	18	16	9	2	1	0	0	0	0	72
Unknown . . .	39	42	34	3	5				8	6	11	7	3	2	2			81
Still Born . . .	70	57	127															127

TABLE NO. 3.

Deaths for the second quarter, at fifteen distinct periods of life.

Under 1 year,	551
1 to 2	294
2 to 5	305
5 to 10	127
10 to 15	46
15 to 20	73
20 to 30	246
30 to 40	263
40 to 50	198
50 to 60	145
60 to 70	112
70 to 80	90
80 to 90	52
90 to 100	5
100 to 110	0
	2507
Still Born	127
Total,	2634

Among the deaths during the quarter, there were from the almshouse 237; people of color, 214; and from the country, 31, as follows:

	Blacks.	Almshouse.	Country.
April,	70	72	11
May,	61	82	9
June,	83	83	11
Total,	214	237	31

TABLE NO. 4.

Meteorological Summary.

	TEMPERATURE.			SNOW AND RAIN.
	Highest.	Lowest.	Mean.	Inches.
April	52.6°	40.6°	46 6°	6.445
May	71.5	55.1	63.3	3.034
June	79.7	63.8	71.7	4.030

The new Registration Law of Marriages, Births, and Deaths.

With the expiration of the present or second quarter of the year, the new "Registration Law" goes into operation throughout the State. All the business, therefore, growing out of the collection and arrangement of the births and deaths in the city of Philadelphia, has been taken from the hands of the Board of Health, and their efficient and gentlemanly clerk, Samuel P. Marks, Esq., who, for many years, has catered to the entire satisfaction

of the profession in these matters, and has been transferred to the office of the "Register of Wills," which officer, has been appointed to carry out the details of the new law, in Philadelphia City and County.

This it will be perceived, is an important change, embracing a variety of new and interesting features, and involving numerous perplexities at the outset. Should the accumulated machinery, that will be required, in order to conduct the enlarged system of registration as proposed in this law, not work with regularity at the commencement, more than usual allowance will have to be granted by those interested in the provisions of the act, until the new officers, who are mere novices in the business, become familiar with their arduous duties. Already, have difficulties and inconveniences arisen to hinder the progress of the measure, and we opine that it will occupy an unlimited period, before uniformity and accuracy in securing and registering Births, Marriages, and Deaths, can be successfully effected.

It is well ascertained that the "Registration law" is by no means a popular measure in the State, as strong efforts were made to defeat the passage of the act in the Legislature. In the interior and elsewhere there are those at this day who would be gratified if it should fail to be carried out. We are not surprised, therefore, to find lukewarmness and tardiness prevailing in those quarters, where we should look for efficient action.

Like all laws of a general character, or, to use a legislative phrase, like all "fancy bills" having no favorite political object to accomplish, nor in any manner affecting the private interests of certain wealthy corporations, there is nothing provided to warm it into existence, and it is obliged to rely upon the cold and disinterested charities of strangers, who, though they have no individual interests to gratify, volunteer to furnish it with healthful nourishment.

We know not otherwise, how to account for the strange and unnecessary delay at Harrisburg in providing the proper forms for the Registers' offices throughout the State. Up to this period, July 22d, the Office in this city, where above all others they are highly necessary, owing to the rapid and constant accumulation of business, the Register, has not received a single

form Book from the Secretary of the Commonwealth; and if we are correctly informed, these books are not yet printed.

Nor has public notice to any extent been given of the precise time when this law goes into operation, nor has any extended information been published, as to the duties and obligations required of those, from whom the information is to be derived.

We speak advisedly for Philadelphia County when we assert, that many of our undertakers, sextons, clergymen, and even physicians and magistrates, have to plead entire ignorance of the requirements of this law, as well as for its existence; so limited has been public information on the subject.

Since the 3d of July, up to which date the Board of Health published their weekly statement of deaths, the Board have declined to receive certificates of Births and Deaths, while the Register of Wills, who has not been fully inducted into this special department of his office, not having had any preparation made for him at Harrisburg to register the returns, has necessarily allowed the whole business to remain a chaotic mass.

We have to regret, that in consequence of the change effected by the operation of this new registration law, we are in danger of losing the weekly statement of deaths, which for years we have been accustomed to receive from the Health Office; nor are we able, from our present information on this subject, to comprehend, or to learn with satisfaction, how or when we shall receive any public account of the births and deaths in our city, other than what is contained in the 13th section of the law; where we are told, that each Register furnishes the Secretary of the Commonwealth with a semi-annual statement, who files the same in his office, and annually transmits to the Legislature an abstract of the births, marriages, and deaths, which have occurred in each county in the State during the year. Consequently, if we are to be supplied with a statement, of the weekly, monthly or quarterly returns of deaths hereafter, we must rely upon private and irresponsible evidence and enterprise; unless the Register can be prevailed upon as a favor, to present a weekly table of the diseases and deaths therefrom, as has been done heretofore. If done at all it must be a favor, as no provision is contained in the law for any other public statement, than that to be made annually through the Legislature.

There are many features in this law, which we could have desired to have been otherwise expressed, but we wish to be satisfied with it as it is, until we have an opportunity to observe its operation, hoping that it may give general satisfaction and eventually confer a favor on the masses. We cannot, however, conceal the fact, that there are many hindrances to its progress; but we know of none, nor need we allude to any other in this connection which ought to cause greater solicitude than the indifference with which intelligent members of our profession treat this important measure. They speak of it as an onerous law, they regard the collecting of statistics, as is provided for in this act, both useless and unnecessary, while at the same time they view the whole plan as one impracticable in its character throughout the commonwealth.

It is true, the change from the old system may burden us with a little more labor, and occupy a little more of our time, than we have been accustomed to allot to the work of furnishing our certificates of births and deaths, but then the object is of such vital importance, that we cannot but hope every medical man who is inclined to find fault with the law, will waive his objections for the general good, as well as for the character of the profession of which he is a member, and give it his hearty approval and support.

We have always entertained the sentiment that the members of the medical profession above all others, should be and would be the firm advocates of a uniform and systematic registration of births, marriages, and deaths; that they are more competent to appreciate its advantages and to be profited by its invaluable results. If the mass of our profession does not sustain this law, who we inquire, will promote its claims? Its successful accomplishment rests with us. The intelligent public, the political economist, the vital statist, and the profession in those States where a system of registration has been introduced and proved successful, are looking on with deep interest to behold the result of a similar measure in this State. Shall they be disappointed?

On the other hand there are those in our own commonwealth, and we hope they are not many, who have from the beginning opposed the passage of this registration law, who will no doubt exult if it should prove a failure, and a failure too from the

supineness or negligence of that profession who should of all others prove its warmest admirers, its undeviating supporters and its best friends.

We sincerely trust, therefore, that the profession throughout the State will, as far as their means and opportunities may enable them, favor in every possible way its claims and its progress, sacrificing private judgment, whenever it can be done without a violation of the conscience, for the general good.

Answer to a Friend on organising the Medical Association. By Samuel Jackson, M. D., formerly of Northumberland.

Messrs. Editors,—One of your correspondents thinks that the exclusion of *permanent members* from the *meetings* of the American Medical Association, would be unjust to them, and taking from them a privilege they have legitimately earned and one that has been accorded to them by the constitution under which they have acted. There is a show of truth in this reasoning, and we shall therefore inquire, by what means our projected exclusion may be justified.

All human associations are kept together, whether in politics, religion, or science, by mutual concessions, and often by injustice similar to that which is now complained of. In the forming of any constitution or code of laws, it is impossible to attain, at once, what is just and best and ought to be permanent. In the year 1838, the Constitution of Pennsylvania underwent a thorough scrutiny and the good old method of appointing judges whether supreme or not supreme, was confirmed and supposed to be established; therefore men of learning and talents, I hope some of them were such, quit the Bar and accepted appointments from the Governor, as they believed, for life. A few years roll by, the people determine to make their own appointments, and behold these men are deposed, I think that in their own *beautiful* language they say *ousted*.

There is nothing *perfect* of man's creating, hence there ought to be in all human associations, a generous spirit of accommodation, which indeed is the very ground-work of all society and of all government. Now where can we hope to find this if it be not

found in the Medical Profession? Above all men in this world, physicians ought to be ready to accommodate and to forget themselves in the promotion of good works. The moment a young man enters the threshold of medicine, he makes a great sacrifice on the altar of benevolence—*he denies himself and takes up his cross.* We may say of ourselves what Shylock says of the Jews, "sufferance is the badge of our nation." We must not therefore keep up a contention for our ultimate rights; to demand the last farthing is ignoble and ungenerous—*de minimis non curat Prætor*, says the Roman code.

But if it appear to a majority of the Association, that the exclusion would be an injustice, let it be only prospective; let the existing members enjoy their privilege; it can last for a few years only; and during this time the meetings will not be so large as to be encumbered by the few that will attend. We are trying to legislate for the future and not for the present generation.

But your ingenious correspondent finds yet another objection to the exclusion bill—*committees and officers are often continued from year to year.* This plea may be very easily disposed of. The same difficulty exists in our Pennsylvania State Society and yet we never found any trouble therefrom; for when the County Societies elect delegates to the State Society, they are careful to choose, among others, those who are officers and committee men. This ought however to be obligatory on them, and I had intended to propose at the last meeting of the State Society, a constitutional amendment to this effect; but there seemed to be no time to spare for things not pressing. Such an amendment might be easily introduced into the constitution of the Association; or it might be decreed that all committee men and officers are *ex officio* delegates to the next meeting.

Your correspondent thinks that it "rests with the proposers of the innovation to justify it if they can." This is sound doctrine and we adopt it without hesitation. We are not much conversant with human affairs and therefore speak with great diffidence; but as far as our knowledge goes, the admission of *permanent members* is an anomaly in human legislation. Does it exist in any other association whether religious, political, or scientific? What interminable wrangling would there not be in

congress, state legislatures, in religious conventions, if the rule obtained! and do you suppose that our physicians are free from the troublesome vice of obtrusive loquacity? If this rule of admission be continued, the time will come and will soon overtake us, when no house will hold both them and the delegates; no voice be heard by the vast assembly; no time will suffice for their ambitious *demonstrations*—I beg pardon for using this word in our native sense.

Another objection is, that some *permanent members* may deteriorate and become totally unworthy of the privilege; they may become such as you would not delegate, such as you would be ashamed to meet. But says your correspondent—"the Association has the power to expel members who have proved themselves really unworthy." Now I should like to know where that power is given. But suppose the constitution were amended to this effect, what a pretty piece of business it would occasion at a meeting of the great Association, where there ought to be nothing unworthy of the *pure physician*, nothing but harmony, good feeling, and love! What, prosecute in this sacred place a poor unfortunate brother for stealing your patient or selling quack medicines! fie, fie. "Don't wash your dirty linen in public."

In what your correspondent calls his "real argument of importance," he makes a most capital blunder. He deprecates "the abolition of permanent membership," and says, "that if those appointed delegates one year, cease with that year to be members of the Association," there would be a difficulty in the appointing of special committees. Now this deprecated abolition of permanent membership has never been proposed. Our plan is, on the contrary, to make every respectable physician in the United States a permanent member as quickly as possible. Look to page seven of our little discourse before the Medical Society and you will find these words—"the privilege granted to what are called permanent members of attending all subsequent meetings and partaking in the debates, is a monstrous anomaly." Now you see that it is not permanent *membership* that we would abolish, but the permanent *privilege* of attending the meetings and making speeches to obstruct the proceedings.

Your correspondent says, "these privileges (of being elected delegates) should be made to rotate amongst all whose characters

and attainments are worthy of a place in the great American fellowship." Very well said—*si sic omnia dixisset.* Of this the County and State Societies will doubtless be very careful, but you must not expect them to prove infallible. Their respect for Professors will always appear, and a full proportion of them will always be elected. It has been proposed, more cunningly than wisely, to allow those schools which live up to a certain *beau-ideal,* to send delegates; and the excellent and authoritative author of this project thinks, that the schools will thus be goaded to a more faithful discharge of their duty. The *good* workings of this began at once, even at Richmond. At the first mention of it, the University of Virginia took fire as by lightning, and exploded, under the direction of a master engineer, with such violence as to frighten a majority out of their wits and to obfuscate others with sand and dust in their eyes: hence they received the said University on its own terms. So much for the first experiment—time will tell the rest, if the new plan should be adopted. Schools will come in from at least three of the four cardinal winds, and the question will arise every year, whether this and that shall be admitted or rejected. Let us advise the anxious schools to bring forward their case in the afternoon, when the members have been saturated with good dinners; for this is the time, according to metaphysicians, to ask a favor. Physicians like other mortals, are fond of good cheer; and that they feel the good effects thereof, let them testify for themselves. At the meeting in Charleston they devoted a marble tablet to commemorate their satisfaction and their "grateful sense of the hospitalities and of the numerous delicate attentions" received: and so profoundly were they impressed with the good and benevolent effects of a full stomach that they *unanimously* resolved to hold the "hospitalities and delicate attentions" among their "most pleasing recollections as long as life and thought shall endure." Now mark, this was done after dinner. Voltaire thinks that a comfortable evacuation has an equally benevolent effect—we leave it to the Association to decide. The author of the above potent resolution, had just offered a pious amendment to the constitution by inserting the words—"in reliance on Divine guidance and support;" but you see it required none of

this extraneous *support* to enable him to recollect in the awful hour of death, the good cheer of Charleston.

At Richmond too, the fascinations of the table received their merited praise, nor did they fail of producing their wonted effects. If the Association were so ungrateful as to decree no marble memorial, nor yet to promise some pleasing recollections of them *in the agonies of death*, some members, more sensible to the blessings they enjoyed, were thrown thereby into such an extatic state as to dance most lustily to the orgies of the Virginian Bacchus, as though they would sweat off the poison of the tarantula. Like Tam O'Shanter's warlocks and witches—

> Hornpipes an' jigs, strathspeys an' reels
> Put life an' metal i' their heels.

Now Messrs. Editors I beg pardon for this long digression, pardon too of my friend Hartshorne for leaving him so long; and if you consider it an intolerable evil, I would recommend a method of deriving from it some compensative good. It was an example in my first grammar, that it is an exceedingly good thing to profit by another's madness—*Optimum est alienâ frui insaniâ.* Whoever then among physicians is afflicted with low spirits, vapors, blues, cold feet, a feeble circulation of blood and spirits, let them go to New York next May, prepare their stomach in the manner of the venerated Ancients, and then put on pumps.

Now for this truly important advice we expect no other reward than to be favored with their "most pleasing recollections so long as life and thought shall endure;" and that when death hovers over them with his sable wings—*cum mors atris circumvolat alis*—they will not forget their humble adviser and friend.

Case of Acute Metritis treated successfully with Urate of Ammonia. By Dr. C. F. Percival.

This article appears to be advancing in public attention, since a communication on its value in this Journal, in regard to Synovial and Serous Inflammations, as the following communication will exhibit.

It appears, also, that it has been tried advantageously in chro-

nic cutaneous diseases and in tubercle of the lung.* This reminds us that it was once the practice in Switzerland to treat consumptives by a residence over the stalls of animals.

Benton, Lowndes Co., Alabama, May 30th, 1852.

PROFESSOR HORNER:

Dear Sir,—On the 25th of April I was called to Dinah Brown, a negress belonging to J. T. Brown, nine miles from my house. On the 15th of March she was delivered of a healthy child. She did well during the month, and on the 19th of April went into the field to labor. She there was caught in the violent storm which occurred on that eve, and walked home fatigued and very wet. About midnight she was taken with violent pains in the uterine region, and about 4 had a violent and excessive hemorrhage. She, however, rallied under the treatment of the plantation midwife, and got rather better, until the 23d, when I was sent for, she being worse. I found her perfectly prostrated, pulse 135, skin hot and dry, at times cold, clammy perspiration breaking out; countenance anxious. Auscultation revealed a creaking sound over uterus, which, enlarged almost to four times its size, was scarcely to be touched from the excessive pain the most careful examination produced; her bowels costive. I placed her under the use of mercurials and opium in two grain doses every three hours, opening the lower bowel by injection of turpentine suspended in white of egg and mucilage, and applied a blister over the whole abdomen. I saw her every few hours, and paid her all the attention a case of this kind requires, occasionally altering my prescriptions. The next morning, the blister having been previously dressed, the tenderness had not decreased much, but she being more comfortable and the secretions having assumed a healthier action, I applied a poultice consisting of our red earth well saturated with urine. I at that time looked on the case as hopeless, although the woman *was* a little better. On Sunday she was improving, and I continued the use of the urinated earth. The next day I left for Virginia, where I had the pleasure of meeting you, but not knowing the result of my case, would not tell it.

On my arrival home I found her well, though still weak. She

* Brit. and For. Med. Chir. Review, July, 1852; from Bucknor's Report, No. 19.

is up and spinning; she had some of the worst symptoms after I left, which were promptly relieved by injections. So much for the Urate of Ammonia. I have often been puzzled at the success I have met by using fresh cow dung poultices in cases of diseases of the knee joint, and have from motives of *fear* kept from relating cases of white swelling which I know were relieved by its use. I first learned it in the County of Armagh in Ireland; that it is of great benefit I know. And that the woman Dinah owes her life to the fact of my having read your paper on Urate of Ammonia in that knee case, is as true. And it does sometimes gladden the heart of a physician to know his efforts have been instrumental in doing good; and as the laurel belongs to you and not to me, I send the case to you, and pray for your prosperity on earth and future happiness in a better and brighter world.

Yours, affectionately,

C. F. PERCIVAL.

BIBLIOGRAPHICAL NOTICES.

Illustrated Manual of Operative Surgery and Surgical Anatomy. By MM. CL. BERNARD and CH. HUETTE. *Edited with notes and additions, and adapted to the use of the American Medical Student, by* W. H. VAN BUREN, M. D., Surgeon to Bellevue Hospital, &c., and C. E. ISAACS, M. D., Demonstrator of Anatomy in the College of Physicians and Surgeons, New York. *Illustrated with steel engravings from drawings after nature, by* M. J. LÉVEILLÉ. *Designed to serve as a companion to the ordinary text-books of Surgery.* New York: H. Bailliere, 290 Broadway. 1852. Part II. (pp. 188 and 27 plates.)

The second part of the American translation of Bernard and Huette's manual maintains in all respects the character established by the first as an excellent reproduction of the French original, and as a beautiful and instructive atlas of Operative

Surgery. It cannot fail to prove a useful adjunct to the different works on Operative Surgery adopted in our schools, and thus handsomely to serve the modest purpose for which it is especially offered to the profession, whilst it may be resorted to with advantage by teachers and practitioners, even in the absence of the text-books.

The present volume begins with eight preliminary plates representing respectively and successively the instruments employed in trepanning, in operating on the eye and the ear, on the larynx and trachea, in the treatment of nasal polypi, in operating on the tonsils and soft palate and on the pharynx and larynx. After these comes Plate 27th, on amputation at the hip joint, in regular continuation of the articular amputations. This is followed by four plates devoted to amputations in the continuity of limbs, beginning with amputations of the foot and hand and ending with amputation of the thigh. Then we have three plates of exsections of the superior and inferior extremities and of the upper and lower jaw. These are succeeded by one on trepanning of the bones of the cranium, (including, as usual, the surgical anatomy of the parts involved in the operation); two on operations on the eyelids; two on the surgical anatomy of the lacrymal apparatus and of the nasal fossæ, the mouth and the pharynx, with the operations performed upon their different ducts and passages; one upon the surgical anatomy and operations relating to the muscles of the eye; one delineating the operation for cataract by depression; another representing the different stages of the operation for extraction, and the various methods of operating for artificial pupil; another representing operations upon the external and internal ear and the surgical anatomy of the parts concerned; two plates devoted to hare-lip, cheiloplasty and other autoplastic operations on the orifice of the mouth; and lastly, by one plate (46th) on rhinoplasty.

It will be seen from the foregoing cursory list that the editors have furnished us in their last instalment (small though it be and beautifully less in comparison with their first) with many of the most important and interesting of the entire series of illustrations. The accompanying text is well arranged, clear, concise and sufficiently full for the purpose intended; and appears to be elegantly and accurately rendered into our vernacular. On every

account therefore we shall hail with interest the speedy completion of the work. It would give us sincere pleasure to view (and *re*-view) our friends' production as a whole—especially in colors. As to these latter we are yet in darkness. Nos. 1 and 2 have come to us in mourning—shorn of half their glory. We trust that the forthcoming portions will throw a better light upon the subject so far as we may be concerned. In this matter of reviewing we confess to some weakness for the "feathers" along with the "fuss"; and when we have to introduce a book to our readers, we expect to see it in the best dress that belongs to it.

The Principles of Surgery. By JAMES MILLER, F. R. S. E., F. R. C. S. E. &c. &c. *Third American, from the second and enlarged Edinburgh Edition. Illustrated by two hundred and forty engravings on wood. Revised with additions by* F. W. SARGENT, M. D., Member of the College of Physicians, Author of Minor Surgery, &c. Philadelphia, Blanchard & Lea, 1852.

The third American, from the second English edition of this standard work of Professor Miller, has been greatly enriched by the notes and bibliography of the edition, so as to present a resumé of all that has been added to the literature of Surgery, since the appearance of the English prototype. Dr. F. Sargent deserves the thanks of the profession for his faithful industry. Of the original work we have already, in the previous edition, expressed our opinion, and need only add our warm recommendation of it to student and practitioner.

An analysis of Physiology; being a condensed view of the most important facts and doctrines. Designed especially for the use of students. By JOHN J. REESE, M. D., Lecturer on Materia Medica and Therapeutics, in the Medical Institue of Philadelphia, &c. &c. Second Edition, revised and enlarged. Philadelphia, Lindsay & Blakiston.

The present edition of Dr. Reese's convenient analysis, has been remodelled by the author, and much of it rewritten; and all that has transpired worthy of note, since the previous issue, has been introduced. As a faithful digest, therefore, it cannot fail to be extremely useful to the student, to whom we cordially recommend it.

THE MEDICAL EXAMINER.

PHILADELPHIA, AUGUST, 1852.

We transfer to our pages, with great pleasure, the following extract from a review of Dr. Gross's work on the Urinary Organs, in the July No. of the British and Foreign Medico-Chirurgical Review, as evidence of a kindlier feeling on the part of foreign journals towards American authors. The reviewer has been speaking of an English work on a kindred subject, by Dr. Coulson, in connection with which he says:

"It has remained for an American writer to wipe away this reproach; and so completely has the task been fulfilled, that we venture to predict for Dr. Gross's treatise a place in the literature of surgery, worthy to rank with the best works of the present age. Not merely is the matter good, but the getting up of the volume is most creditable to transatlantic enterprise; the paper and print would do credit to a first rate London establishment; and the numerous wood cuts which illustrate it demonstrate that America is making rapid advances in this department of art. We have, indeed, unfeigned pleasure in congratulating all concerned in this publication, on the result of their labors; and experience a feeling something like what might animate a long expectant husbandman, who, oftentimes disappointed by the produce of a favorite field, is at last agreeably surprised by a stately crop, which may bear comparison with any of its former rivals. The grounds of our high appreciation of the work will be obvious as we proceed; and we doubt not that the present facilities for obtaining American books will induce many of our readers to verify our recommendation by their own perusal of it."

PIRRIE'S SURGERY.

We received a copy of this excellent treatise too late for notice in this number. In glancing over it, it appears to be admirably executed and well calculated for a text book for students, amongst whom, we doubt not, it will soon become popular.

After we had gone to press with our last number, we learned the death of Dr. JAS. B. ROGERS, Professor of Chemistry in the University of Pennsylvania.

Dr. Roger's health had been failing for some months past, from an attack of albuminuria, which proved fatal under a complication with pneumonia. He was elected professor of Chemistry in the University in 1847, while holding a similar position in Franklin Medical College. He was a popular and successful teacher in his department, and possessed in an eminent degree the love and confidence of all who knew him.

MEDICAL NEWS.

Dr. EDWARD HARTSHORNE has been elected Surgeon to Wills' Hospital for diseases of the eyes and limbs, in place of Dr. Neill resigned.

HOSPITAL OF THE PROTESTANT EPISCOPAL CHURCH IN PHILADELPHIA.—The board of managers have recently appointed the following gentlemen as members of the hospital staff.

Attending Physicians.—Francis West, M. D., Jno. B. Biddle, M. D. Francis G. Smith, M. D., John J. Reese, M. D.

Attending Surgeons.—Wm. B. Page, M. D., H. H. Smith, M. D., B. Henry, M. D., H. Drayton, M. D.

Obstetricians.—Anthony E. Stocker, M. D., Jno. Wiltbank, M. D.

Assistant Physicians.—A. Wilcocks, M. D., R. Clements, M. D., F. W. Lewis, M. D., Moreton Stillé, M. D.

Assistant Surgeons.—A. Hewson, M. D., W. Hunt, M. D., R. F. Penrose, M. D., J. C. Patterson, M. D.

A lot of ground, with a convenient building, has been presented to the Board by Miss Leamy of this city, and it is their intention to commence operations immediately.

THE SAINT JOSEPH'S HOSPITAL.—At a meeting of the Managers of this Institution, on the 12th inst., Dr. Joseph Leidy was unanimously appointed Pathologist to this institution.

Dr. HORNER, Professor of Anatomy in the University of Pennsylvania, has recently met with what may be called an aortico-œsophageal muscle, which arose from the descending thoracic aorta, and proceeding transversely, was attached to the œsophagus about two and a half inches below the division of the trachea. It was about eight lines

wide. He has met in his dissections with two instances of the broncho-œsophageal muscle, of Professor Hyrtl, of Vienna. The aortico-œsophageal belongs to the same category of muscular attachment of the œsophagus to viscera of the thorax.

RECORD OF MEDICAL SCIENCE.

PATHOLOGY AND PRACTICE OF MEDICINE.

Pulmonary Emphysema.—Dr. SELLER read a paper entitled, "On the Production of Vesicular Emphysema in the Lungs, as Explicable by the Principles of Pneumatics."

The object of this paper was to show that the act of expiration must be essentially the cause of vesicular emphysema, in so far as it is a mechanical effect, in opposition to the view upheld by many recent authorities, under which it is regarded as a result of forcible inspiration.

Founding on the acknowledged belief, that in inspiration the air enters each air-cell on the principle of suction, the author concluded, that the inspired air could only become a cause of unusual distension to any portion of the air-cells, if it were found that the inspiratory muscular forces were capable, under given circumstances, of becoming developed to an extent sufficient to overcome a large proportion of the atmospheric pressure exerted on the outer surface of the walls of the chest—a pressure amounting, on a moderate computation, to 10,000 pounds.

To the view adopted by Dr. William T. Gairdner, in his recent papers on "Bronchitis and Bronchial Obstruction," Dr. S. devoted a particular consideration. He represented Dr. Gairdner's view as an assumption supported only by a limited number of facts, to the effect, that vesicular emphysema never occurs unless the dilatability of the lung, by the collapse of portions of its substance, has become less than the expansibility of the chest under the inspiratory forces. Dr. S. described Dr. G.'s view as highly ingenious, and much less at variance with the obvious principles of pneumatics than the rest of the views which ascribe vesicular emphysema to inspiration; but he nevertheless regarded it as untenable, for the following reasons:—1. Because there is great reason to doubt if collapse of a portion or portions of the lungs be uniformly a concomitant of vesicular emphysema; 2. Because it is an unsupported assumption that the chest can expand beyond the present limits to which the lungs can dilate, the contrary being plainly the general rule; 3. Because, were vesicular emphysema so produced, there could be no partial form of the disease, but every part of the lung of one side unaffected with collapse would exhibit a similar state of pathological alteration; 4. Be-

cause, though no unusual inspiratory force, in the case supposed, would be requisite to overcome atmospheric pressure on the exterior of the chest, since, could the enlargement occur, it might take place no faster than the air could enter from without; yet an unestimated additional inspiratory force would be necessary to overcome the resistance of the aggregate of the walls of all the still pervious air-cells, in each inspiration by which a distension beyond their greatest natural magnitude was effected —the possibility of the exertion of such a force being an unsupported assumption; 5. Because, it is not shown, in the cases of vesicular emphysema founded on by Dr. Gairdner, that the aggregate of the distension of the air-cells is in exact correspondence, as to volume, with the aggregate of the decrements which the collapsed portions of the lung have suffered, which should hold, were this view correct.

In support of the view, that expiration is the mechanical cause of vesicular emphysema, Dr. S. referred to forcible compression of the pulmonary air-bag during acts of expiration, great in proportion to the rapidity with which the expiatory forces are exerted, and to the amount of impediment offered at the moment to the free egress of the air by the windpipe; and also to those occasions when, after a very full inspiration, the powerful muscles of expiration violently compress the abdomen and thorax at the same time that the larynx is forcibly closed, so that the air within the lungs is strongly condensed before it is permitted to escape outwards, by the relaxation of the orifice of the larynx. He referred to the frequency of vesicular emphysema in draught-horses, in which it is plainly produced by acts of the latter kind. In proof that pressure of this kind could exercise a distending force on the air-cells when it took place unequally, he referred to the bursting of a bladder by a weight placed upon it, and to the bursting of a paper bag of air when struck at one end. He admitted, however, that the double pressure from without and from within at the same moment, may so far injure the vital texture of the walls of the air-cells, by the compression of the blood-vessels, as to render the distending force more effectual than it otherwise would be.

Dr. W. T. GAIRDNER said, that, notwithstanding the arguments advanced by Dr. Seller, he was still of opinion that the inspiration-theory under the modification which he had advanced in his papers on bronchitis, was the only one which could on mechanical principles account for the production of emphysema. He could not at that meeting enter into the discussion of the exceedingly elaborate paper just read, but he would make one or two observations, and endeavor, at the same time, to place his own view in a simple and clear form before the members. But first, as to the expiration-theories of emphysema in general, he was still at a loss to understand how the force of expiration could ever become a cause of dilatation, either of the whole pulmonary sac, or of a portion of its vesicle. Expiration was essentially a *compressing* force, acting from without, and tending to *contraction* of the air-vesicles; and although the effect of this force might to a certain extent be counteracted or neutralised, when the glottis was closed, by counter-pressure from

within, the amount of this internal pressure was always exactly proportionate to the external, and could, therefore, never act so as to produce distension. The case of the air-filled bladder burst by external pressure, did not, in Dr. G.'s opinion, at all bear upon the question; for, in the first place, the bladder in such circumstances bursts because it is compressed in only one direction, and allowed to expand indefinitely in others, whereas the lung is guarded on all sides by the thoracic parietes, which, in the physiological act of coughing, or other impulsive expiratory acts, prevent its air-cells from being subjected to any kind of distortion of form, such as occurs under the one-sided pressure which produces bursting of the bladder. But, further, the blown bladder, although burst by external pressure, is really never distended, but on the contrary diminished in volume; whereas no one can doubt that in emphysema the air-cells are not so much altered in form as subjected to great distension. The blown bladder is flattened by the force which bursts it; the emphysematous air-cells are permanently expanded, and retain the rounded appearance, even when many of them have been fused into one large bulla by partial destruction of their walls. But, supposing it granted that the expiatory forces might, under peculiar circumstances, act so as to produce partial compression and disorganization of individual air-cells, this would not account for the cases where inordinate distension of the air-cells was observed over a large extent of the lung; still less for the alleged cases of universal emphysema, which had been advanced in opposition to his (Dr. G.'s) theory. The view he (Dr. G.) took of the production of emphysema was this:—Two complex forces acted mechanically on the lung in respiration; the one, that of expiration, a compressing and contracting force as regards the pulmonary air-cells; the other, that of inspiration, an expanding and dilating force. Independently of the arguments already used, he thought it natural to ascribe emphysema, in so far as this was a mechanical lesion, to the action of the latter or dilating force, in preference to the former. It was not difficult to conceive that, as the forces of inspiration have the power, physiologically, to distend their air-cells *up to* their normal maximum, so they might, under certain pathological circumstances, have the power of distending certain air-cells *beyond* their normal maximum. This was what had been assumed to take place by all the advocates of the inspiration theory of emphysema; none of whom, however, had, in Dr. G.'s opinion, correctly generalized the condition of emphysema, or stated the law of its production. In fact, the lung was accurately adapted by nature, in size and form, to the case in which it was enclosed, and the alternate expansion and contraction of which it followed, in consequence of mechanical laws which were understood by every one. No amount of inspiratory power could, in the physiological state, dilate any portion of the lung inordinately, because the thoracic case in which the lung was enclosed was not dilated beyond a certain amount, and the lung was adapted by nature to bear dilation to that amount without injury. But let any portion of the lung be rendered undilatable by disease, *being at the same time diminished in volume*, and it then be-

came possible for the chest to expand so as inordinately to dilate the remaining pervious air-cells, which would tend to occupy the space forfeited by those which were diminished in bulk. Contraction, collapse, or atrophy, therefore, of some portions of the lung, from disease, with inordinate expansion of the sounder portions under violent acts of inspiration, were the conditions under which emphysema was produced. In writing at large on the subject of bronchitis and its consequences, Dr. G. said, he had been at some pains to describe the variety of these lesions observed in the dead body, and to demonstrate by an analysis of cases of emphysema, the coincidence of that affection with the states of the lung leading to partially diminished volume. He had also fully explained the sources of collapse and atrophy of the lung, and directed attention to the great frequency of these affections, and to their connection with, and dependence upon, obstruction of the bronchi in bronchitis and other diseased states. He was, therefore, prepared to state that his theory of emphysema was the fruit of an extensive observation of facts, and he believed it would be found universally applicable, to the exclusion of the expiration-theories, and all the inspiration-theories not founded on the idea of partially diminished bulk in the lung, as the essential cause of emphysema. Other circumstances in the natural history of emphysema were perfectly explained by this theory; as, for instance, its ordinary seat in the anterior edges, the parts least subject to other diseased conditions; its frequency in connection with retrograde or cicatrising tubercle, and its comparative rarity in connection with advancing tubercular disease, and generally with all states of the lung in which large excavations, with flaccid dilatable walls, took the place of the pulmonary air-cells in maintaining the full volume of the lung in the act of inspiration. For the fuller developement of his views, Dr. Gairdner referred to his memoir on bronchitis, and expressed himself much gratified with the intelligent and careful consideration which Dr. Seller had thought it worth while to bestow on his (Dr. G.'s) labors.

Dr. Alison remarked that he was disposed to agree generally with Dr. Seller in believing emphysema to be produced in expiration. He had never been able to understand what force, adequate to the rupture of the walls of the cells, could be supposed to be called into play by any effort of *inspiration*. He did not think that emphysema of the lung was *always* determined by other lesions, as said by Dr. Gairdner; and thought he had seen cases of emphysema affecting the whole lung, at least unattended by such condensation of any part of the lung, as to render it impervious to the air.

Mr. G. Glover commenced by remarking that, in the present transition state of medical science, he was glad to find the author of the paper appealing to acknowledged principles in physics. It was dangerous to proceed practically upon an imperfect knowledge of a few first principles in science; and when applying the principles of pneumatics as "explicable of vesicular emphysema," our ideas must be clear and precise, and the application correct;—not attributing to causes effects

for which they are quite inadequate. The unscientific term "suction," used in the paper, was vague, and often meant nothing, at least nothing upon which correct reasoning could be founded. The enormous pressure attributed to the tendency to a vacuum ("suction") in the chest during tranquil breathing, was greatly overrated. And to show the small amount of the force, he suggested to Dr. Seller the simple experiment of putting one end of a glass tube in water, and the other end in his mouth, during ordinary breathing, when he would observe the water scarcely rising *one inch* in the tube during each inspiration, instead of thirty-three feet, which would be required to represent about fifteen pounds of pressure on every square inch of surface. With regard to Dr. Seller's observation on the bursting of a bladder and a child's paper bag, he stated, that while the paper bag would be easily burst by a sudden percussion, the bladder containing air would sustain hundreds of pounds of pressure before bursting. It is a fundamental principle in pneumatics, that pressure exerted on a fluid must be equal, and in all directions; so that no part of a bladder containing air, and subjected to pressure, could be unequally pressed upon. The bursting pressure would be equally diffused over every part of the surface of the bladder. As an illustration of the principle of equal pressure in fluids, he gave the instance of the early development of the embryo in the uterus, where the most delicate structure floating in a fluid is not ruptured by the violent motions of the mother, or by external violence. He had listened attentively to the paper, but was not prepared to support either theory, regarding the production of vesicular emphysema by inspiration or expiration. If produced by inspiration or expiration, it would be of more frequent occurrence. At the same time, in case of asthma, he would feel more inclined to attribute the production or increase of emphysema to the long and laborious efforts of expiration than the short and easy inspirations. He mentioned that, some days before, he had opened the body of an asthmatic female, where the whole lungs appeared to be emphysematous; and when placed in water they floated very lightly on the surface. Drs. Keiller and Menzies were present at the dissection.

Dr. Alexander Wood was of opinion, that in the discussion, in the anxiety to attend to physical laws, vital laws had been too much overlooked. The natural tendency of every yielding tube was to become stretched when pressed by air on its internal surface. In health the air-cells of the lungs resisted this by their vital elasticity. The tendency of all the diseases with which emphysema was associated was to destroy this elasticity. He doubted the alleged fact on which the new theory was founded—that some diminution in the size of the lungs was found in every case of emphysema. He believed that the forcible entrance of air into the lungs could produce emphysema, as was witnessed as the sequel of too zealous efforts to resuscitate still-born children. He believed that impediments to the exit of free air did the same, or else how was emphysema produced in hooping cough? Not surely by the impeded inspiration, but by the impossibility of the air cells getting

rid of their contents; while, at the same time, the collapsing lung and falling down parietes forcibly compressed it.

Dr. Strachan, of Dollar, in illustration of the applicability of the inspiration-theories to the production of emphysema, referred to the expansion and bursting of a partially air-filled bladder under the receiver of an air-pump, the exhaustion of the receiver producing on the walls of the bladder a similar effect to that exerted by the expansion of the thoracic parietes on the lung.

Dr. Bennet observed that, with respect to vital and physical properties, he considered that the former were so called simply because they could not, in the present state of science, be explained by physical laws, and were peculiar to living beings. The present tendency of physiology was to cultivate physics and chemistry; and in proportion as these explained what was formerly unknown, the so-called vital phenomena diminished in number. No doubt some of these latter, such as contractility in muscle, sensibility in nerve, reproduction, and so on, had not yet been brought into the domain of physics, and hence why these must still be considered as purely vital phenomena. With regard to emphysema, however, the case was different; and it appeared to him that its production, like the phenomena of respiration, was, to a certain extent, explicable by physical laws. In such a case he was unable to understand what Dr. Wood meant by vital, as distinguished from physical resistance of the lung. It appeared to him clear, that if, in the healthy condition, the lungs completely filled the thoracic cavity, and that if, when diseased, a portion of their structure was collapsed, that the sound parts would be expanded by the pressure of the atmosphere, and fill up the vacant space. This was Dr. Gairdner's theory. It had indeed been urged, that the overdistended, or emphysematous pulmonary tissue, occurred without such collapse having been seen. But he considered this to be an example of what frequently occurred in pathological investigation, namely, that such partial collapse of a lung might not be seen, because not looked for. Let a lesion be once carefully described, and the attention of observers directed to it, and a morbid appearance formerly unknown subsequently became very common. This he believed to be the case with partial collapse and condensations of the lung as a cause of emphysema. He had himself remarked the frequency of emphysema in conjunction with certain diseases producing partial contraction and condensation of the lung, as in chronic phthsis. The same fact had been observed by Dr. Ramage, who, in consequence, had erroneously considered that the production of emphysema was the natural cure of tubercular caverns. He also agreed with Dr. Gairdner, as to inspiration being the act of real difficulty in cases of emphysema. The prolonged expiration was principally owing to the impaired elasticity of the pulmonary tissue itself, whereby the air-vesicles were not compressed, and the chief mechanical force during the expiratory act more or less impeded.

Dr. W. T. Gairdner observed, that he scarcely required to add one word in explanation to what had fallen from Dr. Bennet, with which he

entirely agreed so far as the subject of emphysema was concerned. He would, however, remark, that there were three different forms of atrophic lesion, which might give rise to emphysema: the simple mechanical collapse of the lung; the simple chronic atrophy, resulting from that collapse; and the various kinds of atrophy with induration, resulting from pneumonia, tubercle, etc. The last could not be overlooked with moderate attention; but the first might easily be neglected by a superficial observer; and the second, having mostly negative characters, was almost sure to be overlooked by any one, however careful, who came unprepared to the investigation. Since he had given particular attention to the development of emphysema, however, Dr. G. had not found a single instance of that lesion in which he had not been able to demonstrate its connection with one or other form of atrophic disease.—*Trans. Med. Chir. Soc. in London Monthly Journal, May* 1852.

Case of Pseudo-Membranous Laryngo-Bronchitis, or Croup, in an Adult. By M. Bouillaud. *Clinical Remarks on Laryngo-Bronchial Diphtheritis, or Croup, in adults.* By Dr. Bonnet.—The subject of M. Bouillaud's case was a pregnant female, aged 33, of impaired constitution, who was seized, on the 4th February, with symptoms of inflammation of the larynx and bronchi. On the 10th, she miscarried, and soon after died exhausted. A *post-mortem* examination disclosed signs of inflammation in the air-passages, in which were found also a quantity of false membrane. During her illness she had coughed up some casts of the trachea and bronchi; and M. Bouillaud thinks that death was rather due to the exhaustion produced by the miscarriage.

Dr. Bonnet relates two cases, also occurring in pregnant females. One of them died; the other recovered, and subsequently gave birth to a living infant. He does not think that diphtheritic exudation is always connected with inflammation. The only rational treatment is by emetics, which act, first, by causing mechanical expulsion; and secondly, by augmenting the secretion of the air-tubes, so as to favor the softening and removal of the false membrane. Their production of diaphoresis is also valuable. In cases of imminent danger, Dr. Bonnet would perform tracheotomy.—*London Journal of Medicine, June*, 1852.

Case of Spasmodic Constriction of the Œsophagus. By Dr. B. Palais.—The patient was a young man, aged 21, who had intermittent fever. Dr. Palais was called to him in the hospital at Montmorail, on December 22, 1851, where he found him in the following condition:

The skin was in general cold; there was trembling of the whole body; the pulse was 60; the jaws were contracted; deglutition was impossible, and liquids produced severe pain in the pharynx, and caused painful efforts to swallow, which congested the face; there was difficulty in protruding the tongue; the front of the neck was tender, and the movements of this part were impaired, while the limbs and back could be bent; the urine was scanty; the patient tossed himself about in bed; he understood questions, but could only answer them imperfectly. Sina-

pisms were applied several times to the lower limbs; warmth was applied to the body; and ether mixture was given; and the patient was bled from the arm.

December 23. The same state continuing, the bleeding was repeated.

December 25. The patient was calm; he could swallow a little, but with difficulty, so that it seemed as if the fluids were passing through a filter. The pulse was 72; the skin was regaining its normal heat. He complained of pain at the upper part of the sternum, and also between the lower ribs on the left side. Nothing abnormal was detected in the thorax by auscultation.

December 28. The dysphagia had gradually ceased, and there now appeared slight attacks of ague, which continued till the 31st, and then yielded to sulphate of quinine.—*Ibid.*

On Diphtheritis of the glans in some cases of paralysis. By Dr. Hérard.—In two persons, who had been the subjects of cerebral hæmorrhage followed by hemiplegia, Dr. Hérard found a morbid exudation at the extremity of the glans, round the urinary meatus. In both cases, there was incontinence of fæces and urine; and the attention of Dr. Hérard was directed to the glans by the patient complaining of pain at the extremity of the penis. The exudation was removed by careful cleansing, and the use of acid nitrate of mercury.

Dr. Hérard thinks that the origin of the diphtheritic eruption is sufficiently explained, by the penis having been constantly plunged in a metallic vessel containing decomposed urine, and being thus exposed to the emanations therefrom.—*London. Journ. Med.*

Some facts in Pathological Anatomy regarding the state of the spleen in the intermittent fever of Madagascar. By Dr. Rochard.—Dr. Rochard had an opportunity of observing a large number of severe cases of intermittent fever in Madagascar, in 1830. He gives the following particulars regarding the state of the spleen.

In 153 carefully performed necropsies, he found the spleen diminished in 31 cases; in 22 of these, the patient had not taken the smallest quantity of sulphate of quinine. In the remaining 9, there was nothing abnormal in the state of the spleen, before quinine was given. In 122 patients who took sulphate of quinine in average and daily doses of a gramme (15½ grains) for several days, the spleen always became greatly enlarged: and the same happened with some who had taken no quinine. The enlarged and softened organ exhaled an odour resembling that proceeding from the marshes.

From his observations, Dr. Rochard concludes that—1. The spleen is not the starting point of intermittent fevers. Like all internal organs, it may be congested: and very often its volume, instead of being enlarged, is notably diminished.

2. The frequent increase in size of the spleen is due to its spongy tissue permitting it to become more gorged with blood, under the influence of febrile congestion.

3. Sulphate of quinine does not always diminish the size of a congested spleen.

4. The individual varieties which the size of the spleen presents in man or in animals, in health or in disease, prevents us from appreciating in an exact manner the influence of sulphate of quinine or chloride of sodium in diminishing its volume.

5. Antiperiodic remedies arrest the febrile attacks, not by diminishing the size of the spleen, but by modifying, in a special manner, the economy in general. This modification brings about a state of reactionary equillibrium, under which the miasmatic influence is eliminated from the body.—*Ibid.*

Obstinate intermittent coryza—instantaneous cure. By Dr. A. Menudier.—A lady, aged 24, had been for some years subject to severe attacks of coryza, coming on twice or three times in a week, and lasting from twelve to thirty-six hours. On one occasion, she was anxious to have the disease arrested immediately. M. Menudier thereupon applied a sinapism from the scapulæ to the loins : in a quarter of an hour, the coryza began to diminish, and in three quarters of an hour the patient could no longer bear the mustard. The skin was reddened; but the coryza was cured; and had not returned at the end of three months.—*Ib.*

OBSTETRICS.

On Abdominal Tumors during Pregnancy. By Dr. Morisseau.—On two occasions, Dr. Morisseau writes, I have had an opportunity of observing the result of abdominal tumors during pregnancy. More recent facts have awakened my recollection, and fixed my attention.

A woman, aged 28, married for eight months, had become pregnant four months after marriage. For several years she had had a swelling on the right side of the abdomen. It was painless and moveable, offering no impediment to the functions of the abdomen ; but it now became adherent and painful. In the seventh month, the woman died of marasmus. For a month before her death, she had had purulent alvine evacuations; but the size of the tumor appeared very slightly diminished. The tumor was found to be filled with pus, to be adherent to the mesentery, and to have an opening into the ascending colon. The fœtus was in a state of putrefaction.

A woman, aged 30, who had had two children, became pregnant for the third time. After the second pregnancy, she had what she described as a ball in her abdomen. This tumor went on growing with the product of conception ; and, in the fifth month, became so painful as to render motion insupportable. After suffering extremely for four months, in a state of marasmus, she was delivered of a small, living, skeleton-like child. Both mother and infant died a few hours after. Towards the eighth month of pregnancy, I perceived a tumor of the size of the head of a fœtus at full term, seated in the right flank, and distinct from the uterine tumor. At the autopsy, the peritoneum was

found to contain a large quantity of sero-purulent fluid, with albuminous flocculi floating in it. The small intestines were mottled and friable. Above the right ovary, the tumor raised the intestines, and was adherent to them by the whole of its anterior surface. It was divided into cells, which, for the most part, did not communicate, and was filled with a large quantity of very fetid greenish pus. The base was at the mesentery. The uterus appeared healthy.

Some time ago I was consulted in the case of a woman, aged 24 or 25 years, who had been married some months, to determine whether she could, without danger, become pregnant. She had for some years had an enlargement in the abdomen. The tumor was of the size of a large orange, painless, and could easily be moved from the right side, its ordinary position, to the left. I expressed my opinion that pregnancy would be attended with severe accidents; but different advice was given by another *confrere*, and the patient became pregnant. A month after conception, the tumor became painful, and larger. Towards the end of the second month the patient was obliged to remain in bed; not the least pressure could be borne; obstinate vomiting and colic set in; and, for ten days, the patient was in a most precarious state. On the seventieth day of pregnancy she expelled the fœtus. For a month she remained dangerously ill, but at length became slowly convalescent, the tumor remaining large and tender.

These facts are worthy of attention. Pregnancy necessitates increased activity of circulation in the uterus and the neighboring parts. When the uterus is developed, it impedes the flow of blood, as is proved by œdema, and varices in the lower extremities. In the abdomen, too, there is a plethoric state of the organs; hence, if a ganglion is already hypertrophied, or there be a tumor of any kind in the abdominal cavity, it will be liable to become enlarged.

Is it, then, prudent to advise marriage to a female who has a tumor in the abdomen? I have not hesitated, and would not hesitate, to reply in the negative.—*Lond. Journ. of Medicine.*

SURGERY.

New Mode of Reducing Strangulated Hernia.—Dr. Wise makes the following interesting communication in a letter addressed to Professor Syme:—

"The following are the particulars I promised to send you, regarding a new method of reducing strangulated hernia. While I had charge of an hospital in India, an elderly man was brought to it with a strangulated inguinal hernia. After in vain employing the usual means of reduction, I was preparing to liberate the gut with the knife, when a Mussulman gentleman suggested that the following method should be first tried, as he had seen it successful. As it appeared most simple and effective, I at once proceeded to try it. The patient was placed upon a table, and a long sheet, folded several times on itself, was carried round the lower part of the abdomen of the patient, was twisted on itself

in front, and again on the sides, so as to enable an assistant, standing on each side of the patient, to hold the extremities of the sheet, and to pull them gently upwards, or towards the patient's head, while a third assistant held the feet steady, and the surgeon used the taxis.

"As the gut immediately above the strangulated portion was superficial and distended with air and liquid, it was drawn upwards with considerable force from the hernial sac, which was assisted by the surgeon using the taxis; when the strangulated portion was immediately reduced.

"This simple method may, in a very large proportion of cases, be employed with perfect safety and at an early period, before inflammation and thickening has complicated and increased so much the danger of the operation, which is thus rendered unnecessary."—*Ibid.*

MATERIA MEDICA AND THERAPEUTICS.

On the Action of Chloroform. By M. Hervez de Chegoin.—M. Hervez de Chégoin thinks it probable that chloroform may in some cases exert a deleterious influence on the organs, short of death; and he relates a case of amputation of the thigh, in which anæsthesia was produced, and in which both the mucous membrane and the divided surface of the muscles presented a blue appearance. The bone became denuded of periosteum, the suppuration became unhealthy, and the patient died. He would not, however, give up the use of chloroform; but would employ it until the patients manifested a desire to remove themselves from its influence, pushing it away with the hand, at the same time that they have a noise in the ears, and they feel a pinch but obtusely. He has operated on several occasions under these circumstances, the patients not being aware of the operation.—*Ibid.*

CHEMISTRY.

On the Use of Barium, Cadmium and Nickel in Pharmacy.—Barium and its compounds, more particularly the chloride of barium (which, by the bye, has been struck out of the text of the new *Pharmacopœia Londinensis*, and merely placed among the tests in the Appendix,) is just now attracting the attention of physicians, as applicable to those diseases in which the iodides of iron and of potassium, and the tincture of sesquichloride of iron, have been administered with such decided advantage.

The salts of two other metals are likely to come into considerable use as articles of the *Materia Medica*. The metals alluded to are Cadmium and Nickel, of which the sulphates have been lately employed with advantage by some distinguished physicians, as serviceable therapeutic agents, more particularly, it is said, in some diseases peculiar to females.—*Ibid, from Annals of Pharmacy.*